AN ANALYSIS OF DISCIPLINARY, POLITICAL AND LEGAL INFLUENCES ON THE PRACTICE OF NATURAL RESOURCES SCIENCE

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An Analysis of Disciplinary, Political, and Legal Influences on the Practice of Natural Resource Science

03-JV-269
Final Report

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Overview

This report and accompanying materials constitute the final report for 03-JV-269. The purpose of the research joint venture agreement was for the principal investigator to collaborate with Daniel R. Williams of the Rocky Mountain Research Station on co-authoring a series of papers which explore the institutional context in which science operates. The project produced 1 published peer review article, 1 published book review, 1 article currently undergoing review, 3 conference presentations, a policy review article to be submitted for review (summer 2007), and an annotated bibliography. All of these products are briefly summarized below and papers/draft papers are attached as part of the overall project report. (Note conference presentations are not attached.)

Summary of Final Report Components

Peer Review Publications

(1) Patterson, M. E., & Williams, D. R. (2005). Maintaining research traditions on place: Diversity of thought and scientific progress. *Journal of Environmental Psychology, 25*(361-380).

Since the 1990s, numerous authors have expressed concerns about lack of conceptual clarity in research on place. Some authors suggest that place research has failed to evolve into a systematic and coherent body of knowledge. We believe recent critiques do not adequately characterize the state of knowledge in place research, but responding to the issues raised requires investigating epistemological foundations of place research traditions. Specifically, seeing systematic coherence requires a pluralistic world view that understands place, not as a single research tradition but as a domain of research informed by many disciplinary research traditions at the research program and paradigmatic level. This paper introduces a framework for discussing epistemological foundations of research traditions then uses it to: characterize the body of place research, analyze recent critiques regarding the state of place research, make a case for the value of diversity in thought, and explore the notion of scientific progress in relation to place research.

(2) Williams, D. R. & Patterson, M. E. (in review). Snapshots of what, exactly? A comment on methodological experimentation and conceptual foundations in place research. *Society and Natural Resources*

Place ideas in natural resource management have grown in recent years. But with that growth has come greater complexity and diversity in thinking and mounting confusion about the ontological and epistemological assumptions underlying any specific investigation. Beckley et al. (2007) contribute to place research by proposing a new methodological approach to analyzing attachments to place and exploring the relative importance of biophysical versus socio-cultural attributes in determining place attachment. While our thinking has benefited from their contributions to place research, we see an increasing need to clarify the multiple and competing paths for place research easily obscured in the heap of similar sounding place concepts. Our commentary cautions against philosophically unguided methodological experimentation and offers some critique of their conceptual approach to place.

(3) Patterson, M. E., & Williams, D. R. (in prep). Policy for Science: Negotiating the Changing Social Contract in the US. *Society and Natural Resources*

Recent legislative mandates regarding policy for science should be understood as reflecting a societal debate that runs deeper than merely than attempts by special interests to subvert science through political avenues. Institutions are created in social contexts in response to the values, goals, and conflicts in a particular culture at a given point in time. But over time, the original social contexts that give rise to institutions change as technology, values, goals, and meanings change. As a result, institutions must also evolve and adapt if they are to continue to function successfully in that culture. Science is not an exception, it evolves as a result of both external and internal pressures. Contemporary external pressures, stemming from the first two dialogs reviewed in the paper in which society is seeking to place appropriate checks and balances on administrative agencies as part of our democratic system of governance (dialog #1) and to renegotiate its relationship with science as a publicly funded good (dialog #2) , are leading to a rise in policy for science initiatives. These have implications for the practice of science. Internal pressures stem from the emergence of a more pluralistic perspective on scientific epistemology reflected in the third dialog. A key professional challenge becomes how to keep communities of specialist scientists integrated with the societies that support and benefit from their work. We believe the questions of how to provide sufficient understanding and involvement in these evolving dialogs within the scientific community is an area that needs greater attention within the profession. This article seeks to contribute toward this effort by outlining the different social dialogs that are converging around these issues.

Book Review

Patterson, M. E. (2005). Book Review: Nature's experts: Science, Politics, and the Environment. The Quarterly Review of Biology, 80(4), 498-499.

Conference Presentations (only the abstracts included, papers not attached)

(1)Patterson, M. E., Montag, J. M., & Williams, D. R. (2005). Assessing social debates in wildlife conflicts: The question of predator compensation. In From Knowledge to Management: Balancing Extraction, Protection, & Experiences. The 11th

International Symposium on Society and Resource Management Book of Abstracts. (pp. 39). Ostersund, Sweden.

Social conflict over wildlife management is increasingly prevalent due to a variety of factors including urbanization, a trend toward increasingly individualized meanings of wildlife in postmodern society, and efforts to reintroduce predators. One challenge for decision makers embroiled in wildlife conflicts is how to construct an understanding of what the social debate regarding controversial wildlife issues is really about. In the United States, there has been growing interest in collaborative strategies as a means to generate understanding and to navigate solutions. The turn toward collaborative discourse as a means of resolving conflicts raises the question of what role, if any, social science should play. We maintain there is still a prominent role for science-based social assessments in a collaborative approach. However, we believe that the nature of social research most useful in facilitating collaborative goals will differ from the social psychological/attitude based models that have historically dominated human dimensions in wildlife research (at least in the USA). While there has been great success in measuring attitude constructs, the statistical properties and nature of these types of psychometric measures are not adequate for assessing the social debate over issues like predator compensation. This is because these debates entail a host of qualitative, context dependent factors rather than separate and independent constructs. For measurement of the latter, human dimensions research is increasingly turning to case studies, interpretive methodologies, and qualitative forms of measurement. This paper illustrates an interpretive strategy used to explore the nature of the social debate about predator compensation programs in three western states. The analysis suggests that the debate regarding compensation is better understood in terms of concepts such as equity and how costs should be distributed in society than in terms of differences in views about wildlife.

(2)Patterson, M. E., & Williams, D. R. (2006). Legislating science? Trends and implications of recent policy-for-science mandates. In Social Sciences in Resource Management: Global Challenges and Local Responses. The 12th International Symposium on Society and Resource Management Book of Abstracts. Vancouver, BC, Canada.

In natural resource management the relationship between science and governance typically is viewed in terms of the role of science in generating policy. When influence flows in the opposite direction, concerns about inappropriate politicization of science are quickly raised. However, in the United States, recent years have seen a growth in this latter trend through increased legislative and administrative policy for science. These include the Shelby Amendment, the Data Quality Act (both riders to appropriations bills), and the Federal Policy on Research Misconduct (from the Executive Office of Science and Technology Policy). We argue that these recent legislative mandates regarding policy for science should be understood as reflecting societal debates that run deeper than merely attempts by special interests to subvert science through political avenues. Rather they represent the convergence two historical and evolving discourses about the relationship of society to science. One discourse stems from the Progressivist vision for an administrative state and the other from the "social contract for science" emanating

from Vannevar Bush's1945 report, *Science: Endless Frontier*. A new social contract is for science is being negotiated, one that has the potential to redefine not only the relationship between science and governance, but also the very nature of science. Thus it is important for natural resource scientists to be active participants rather than passive bystanders. However, engagement in these negotiations by natural resource professionals has been minimal and sporadic. This paper seeks to stimulate more active and deliberative professional engagement in trends related to policy for science.

(3)Patterson, M. E., & Williams, D. R. (2007). Contributions of Social Science to Natural Resource Management: Can Interpretive and Qualitative Science Withstand Judicial Scrutiny?. The 13th International Symposium on Society and Resource Management Book of Abstracts. (pp. 39). Ostersund, Sweden.

Science is often a highly contested issue in debates over management of natural resources. Two recent trends relevant to this issue are likely to further heighten managerial concerns about the viability of science conducted to support agency decision making. The first stems from the growth of "policy for science" mandates by Congressional and executive branches including: the Shelby amendment, the Data Quality Act, and the OMB Bulletin for Peer Review. Much like the passage of major natural resource legislation such as the NEPA, the NFMA, and the ESA, these policies heighten the opportunity for external parties to review and challenge the use of science in agency decision making. At the same time, there is an emerging trend within science toward a more pluralistic view of what science is. Among other changes, this more pluralistic view has led to an increase in critical, interpretive, and qualitative research approaches in the social sciences that require re-evaluation of traditional scientific norms within the natural resource profession. Faced with research that may require re-evaluation of traditional scientific norms on the one hand and the possibility of increased external scrutiny arising from recent policy for science initiatives on the other, managers are understandably concerned about whether qualitative and interpretive research satisfies NEPA's "hard look" requirement and other legal standards. This paper analyzes several recent court rulings that indicate qualitative and interpretive research can withstand judicial scrutiny.

Annotated Bibliography

Patterson, M. E. (2006). The Generation, Management, Migration, and Evaluation of Knowledge Within and Across Communities of Practice. An Annotated Bibliography. College of Forestry and Conservation. University of Montana, Missoula, MT.

Knowledge comes in a variety of forms (e.g., wisdom, science, information, outformation (Ezrahi, 2004)) and resides in a variety of different institutional contexts (e.g., universities, medicine, law, administrative decision making). Across these institutional contexts, the generation, management, use and standards of evaluation for knowledge, though similar in some respects, also differs. Even within fields of practice, there are different subcultures that value different types of knowledge and have different validity

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Maintaining research traditions on place: Diversity of thought and scientific progress

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Abstract

Since the 1990s, numerous authors have expressed concerns about lack of conceptual clarity in research on place. Some authors suggest that place research has failed to evolve into a systematic and coherent body of knowledge. We believe recent critiques do not adequately characterize the state of knowledge in place research, but responding to the issues raised requires investigating epistemological foundations of place research traditions. Specifically, seeing systematic coherence requires a pluralistic world view that understands place, not as a single research tradition but as a domain of research informed by many disciplinary research traditions at the research program and paradigmatic level. This paper introduces a framework for discussing epistemological foundations of research traditions then uses it to: characterize the body of place research, analyse recent critiques regarding the state of place research, make a case for the value of diversity in thought, and explore the notion of scientific progress in relation to place research.

1. Introduction

In a variety of disciplinary fields (including architecture, environmental psychology, human geography, and sociology among others), the concept of place (also variously referred to as sense of place, place attachment, and place identity) has emerged as a prominent focus for exploring the relationship between humans and the environment. Invigorated by the emergence in the late 1960s of a humanistic critique in geography, the concept gained prominence among phenomenological researchers in architecture and geography in the 1970s with the publication of work such as Norberg-Schulz's (1980) *Genius Loci: Towards a Phenomenology of Architecture*, Relph's (1976) *Place and Placelessness*, and Tuan's (1977) *Space and Place: The Perspective of Experience*. Interest in the concept of place was initially slow to spread beyond phenomenological researchers and humanistic geographers due to the dominance of quantitative and positivistic philosophies in environment and behavior research (Low & Altman, 1992). Over the last two decades, however, place has attracted considerable attention from researchers in a variety of research traditions.

Low and Altman (1992) suggest that evolution of concepts like place within the social sciences often follows a common trajectory. In the first stage, scholars treat a new concept as if there is a consensus about its meaning. The second stage is initiated by an erosion of this presumed consensus. Scholars then debate the meaning of concepts with greater rigor, developing taxonomies to characterize different but often related phenomena encompassed within the original concept in a quest for conceptual clarity amid the diversity of interpretations. The third stage involves "development of systematic theoretical positions and clearly delineated programs of research and application of knowledge to the solution of practical problems" (p. 3).

By the 1990s, place research entered a stage where there appears to have been a substantial erosion of consensus. In the last decade, numerous authors have raised questions about lack of conceptual clarity. Various authors have noted a proliferation of specific concepts (e.g. genius loci, place, sense of place, place attachment, place identity, place dependence, rootedness, topophilia) which they perceive to

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have vague and fuzzy definitions (Lalli, 1992; Shamai, 1991), reflecting something more aptly described as ideas than well-defined constructs (Kaltenborn, 1998), for which distinctions and linkages among concepts have been inadequately specified (Hammitt, Backlund, & Bixler, 2004; Hammitt & Stewart, 1996; Jorgensen & Stedman, 2001; Manzo, 2003; Stedman, 2002). Hidalgo and Hernandez (2001) have suggested that the variety of disciplines from which place research is now approached has helped to create a situation in which there is little agreement regarding the name of the underlying concept, its definition, or what methodological approach is best suited to its study.

Researchers raising this concern over the last decade frequently have suggested that the resolution of this problem will come through attempts to develop constructs that can be operationalized (Jorgensen & Stedman, 2001; Kaltenborn, 1998; Lalli, 1992; Shamai, 1991; Stedman, 2002). However, this suggested solution does not appear to meet criteria laid out by other place researchers. For example, Relph (1976), one of the phenomenologically grounded pioneers in the development of the concept of place, early on expressed the view that place "is not just a formal concept awaiting a precise definition ... clarification cannot be achieved by imposing precise but arbitrary definitions" (p. 4). Similarly, Seamon (1987) suggests that attempts to operationalize place-related concepts into constructs like place identity eliminate the "phenomenological essence of place as a psycho-social-environmental whole larger than the sum of its parts" (p. 20) resulting in a superficial treatment of the underlying phenomenon. In other words, some of the recent recommendations for how to achieve conceptual clarity appear to contribute to continued erosion of consensus rather than resolve it.

In addition, various authors express different views about the status of place as a theoretical concept and different visions about what Stage III of Low and Altman's (1992) postulated progression should yield. Bonnes and Secchiaroli (1995) suggest the initial diversity that characterized environmental psychology has evolved into one of greater integration among theoretical perspectives through the introduction of new concepts one of which is place. While they describe place as "a nonunivocal theoretical perspective" (p. 161), they suggest the possibility of integration across diverse perspectives and refer to development of "the theory of place" (p. 197). Stedman (2002, 2003) interprets the current lack of consensus as a failure in the progression of place research according to the model of concept evolution outlined by Low and Altman (1992). Stedman and Jorgensen suggest that progression can be achieved by translating "place terminology into social psychological concepts with well-established measures" (Stedman, 2002, p. 561) that permit quantitative hypothesis testing; specifically an attitude framework (Jorgensen & Stedman, 2001; Stedman, 2002, 2003). Franck (1987), in contrast, suggests that differences in the goals and assumptions underlying alternative research traditions in place are so great that integration is not a possibility. Finally, in summarizing his review of place research in sociology, Gieryn (2000) concluded that the domain of study was unbounded and could not be summed up into a neat propositional inventory of empirical findings. He suggests that ultimately, place should not be seen as a distinctive kind of explanatory model, but rather more generally as a way of doing sociology.

The recent critiques described above and the diverse visions about the appropriate path of maturation in place research raise fundamental questions about the nature of research. How does science progress? How does one evaluate progress in the development of theoretical concepts? Is diversity in perspective and approach bad (an indication of lack of conceptual clarity)? Is one epistemology arguably superior to another? Is integration across perspectives possible? Are standardization and integration desirable?

In response to these questions, we believe that the mere existence of diversity in perspective and approach does not mean the development of the systematic and rigorous Stage III body of knowledge anticipated by Low and Altman (1992) has not been achieved. In our view, recent critiques suggesting lack of conceptual clarity and lack of systematic progression results from viewing place research as if it should constitute a single research tradition. Instead, we maintain that it is more appropriate to view place as a domain of research informed by multiple research traditions. Adopting this latter vantage point puts researchers in a position to see greater coherence and conceptual clarity across the body of place research than recent critiques suggest. However, the willingness or capacity to adopt this vantage point requires embracing a normative stance on science about which there is substantial debate. The dimensions of the debate involve questions about (a) the adequacy or scientific merit of divergent epistemologies; (b) how to deal with diversity in perspectives (i.e., through opposition, integration, or reflective dialog); and (c) the requirements of Stage III research, which seeks to translate conceptual and empirical knowledge into the realm of practice. Addressing these issues requires a framework for exploring epistemological foundations of research traditions that transcends disciplinary boundaries. Below, a framework developed from literature in the philosophy of science is introduced and used as the basis for organizing the discussion to characterize the body of place research, to analyse recent critiques regarding the state and progression of place research, and to make a case for the value of diversity in thought in place research.

2. The nature of epistemological research traditions

Epistemological research traditions are complex and fluid phenomena. Characterizing their nature, therefore, has always been a difficult task, subject to pitfalls such as creating straw men caricatures (in cases where authors characterize traditions they do not subscribe to) or of

reifying a set of rules that do not truly describe how a specific epistemological tradition really works. However, as social phenomena, all scientific research traditions have a structure and philosophers of science have devoted a great deal of attention to the task of how to characterize them. Since the work of Thomas Kuhn, philosophers of science have conceived of the appropriate unit of analysis for epistemological traditions to be their macrostructure (Anderson, 1986). There is no single, universally adopted framework for analysing and characterizing the macrostructure underlying epistemological traditions. However, one proposed by Patterson and Williams (1998) building on work by Laudan (1984), Anderson (1986), and Murray and Ozanne (1991) utilizes a multi-layered framework comprised of three levels (Research Programs, Paradigms, and World Views) as a basis (Fig. 1). This framework provides a useful foundation for organizing a discussion that examines and responds to the recent critiques of place research.

As an element of the macrostructure, research programs are the site of actual application of science, where theoretical concepts are developed and empirically tested and where traditional disciplinary foundations (e.g. environmental psychology, geography) are most active. Therefore, this is the level at which scientists are most familiar and comfortable. Discussions of research programs are typically organized within a discipline according to either different conceptual schools of thought or different substantive concerns within the discipline. Stephen and Rachel Kaplan's (Kaplan, 1992; Kaplan & Kaplan, 1989) evolutionary/information-based approach to environmental preference, Richard Stedman's and Brad Jorgensen's (Jorgensen & Stedman, 2001; Stedman, 2002, 2003) attitudinal approach to studying sense of place, and Randolph Hester's (1993) research on the sacred structure in relation to community planning are examples of research programs.

Research programs are linked to paradigms. As a dimension of a research tradition, paradigms are the site where normative philosophical commitments that guide an approach to research (i.e. guide the development and testing of theoretical concepts within research programs) are established. Normative commitments reflect philosophical assumptions about issues such as human nature and the nature of reality (ontology), the nature and process of knowing (epistemology), and the terminal and instrumental goals of science (axiology). Behaviorism, critical theory, ethnography, grounded theory, hermeneutics, phenomenology, and psychometrics are examples of paradigms. Paradigms often transcend disciplinary boundaries. For example, it is possible to find both general and discipline specific (e.g. anthropology, psychology, geography, etc.) discussions of phenomenology (Seamon, 2000).

World views inform paradigms. This is the level at which individuals' concept of science resides; where people debate the nature of science. It deals with broad philosophical debates about the appropriate concept of validity and the nature of scientific testing logics (a logic explaining the manner in which empirical observations function as tests of theoretical concepts). Fundamental questions related to diversity, integration, and superiority of divergent epistemologies are explored at this level.

Issues at all three levels of this framework are evident in recent critiques of place research and in different visions of what the state of knowledge should look like in relation to Low and Altman's (1992) Stage III research. Specifically, examining place research at the Research Program level provides a basis for systematically characterizing distinct conceptual traditions in a manner that reveals greater conceptual clarity and coherence than recent critiques seem to acknowledge. Analysis at this level provides a means to consider possible costs of reducing the diversity in conceptual traditions through adopting one particular research program as the basis for future place research (a strategy suggested by some recent critiques). Also, viewing place research at this level provides an opportunity to make the important distinction between variation in use of terminology within a research program versus across research programs. Exploring place research traditions from the Paradigmatic level reveals how different philosophical commitments often lead to incompatible methodological directives and how this has contributed to arguments regarding lack of conceptual clarity in place research. Analysis at this level also reveals how paradigmatic commitments shape the meaning of terms. The same term can have very different meanings in different paradigms. Providing concise definitions of terms of the sort many recent critiques seek is often not possible because a full understanding of key paradigmatic concepts often requires a comprehensive understanding of the system of philosophical commitments in which they are used. Finally, exploring place research traditions at the World View level reveals how different stances on science have contributed to recent critiques of place research. It also provides a basis for considering the consequences of adopting different stances (oppositional, integrative, and reflective) to the diversity and divergence apparent at the Paradigmatic level. The relevance of these issues to recent critiques of place research is discussed more fully in the following sections.

2.1. Critiques of place research at the Research Program level

Many of the recent critiques of place research have focused on concerns with respect to inadequate theoretical development and coherence, issues typically addressed at the Research Program level. For example, Lalli (1992) describes "differing theoretical foundations and fragmented formulations" as a problem in research on place identity. Similarly, in noting the diversity of disciplines in which the concept of place has been explored, Hidalgo and Hernandez (2001) suggest that one of the problems blocking progress is that there is no agreement about the

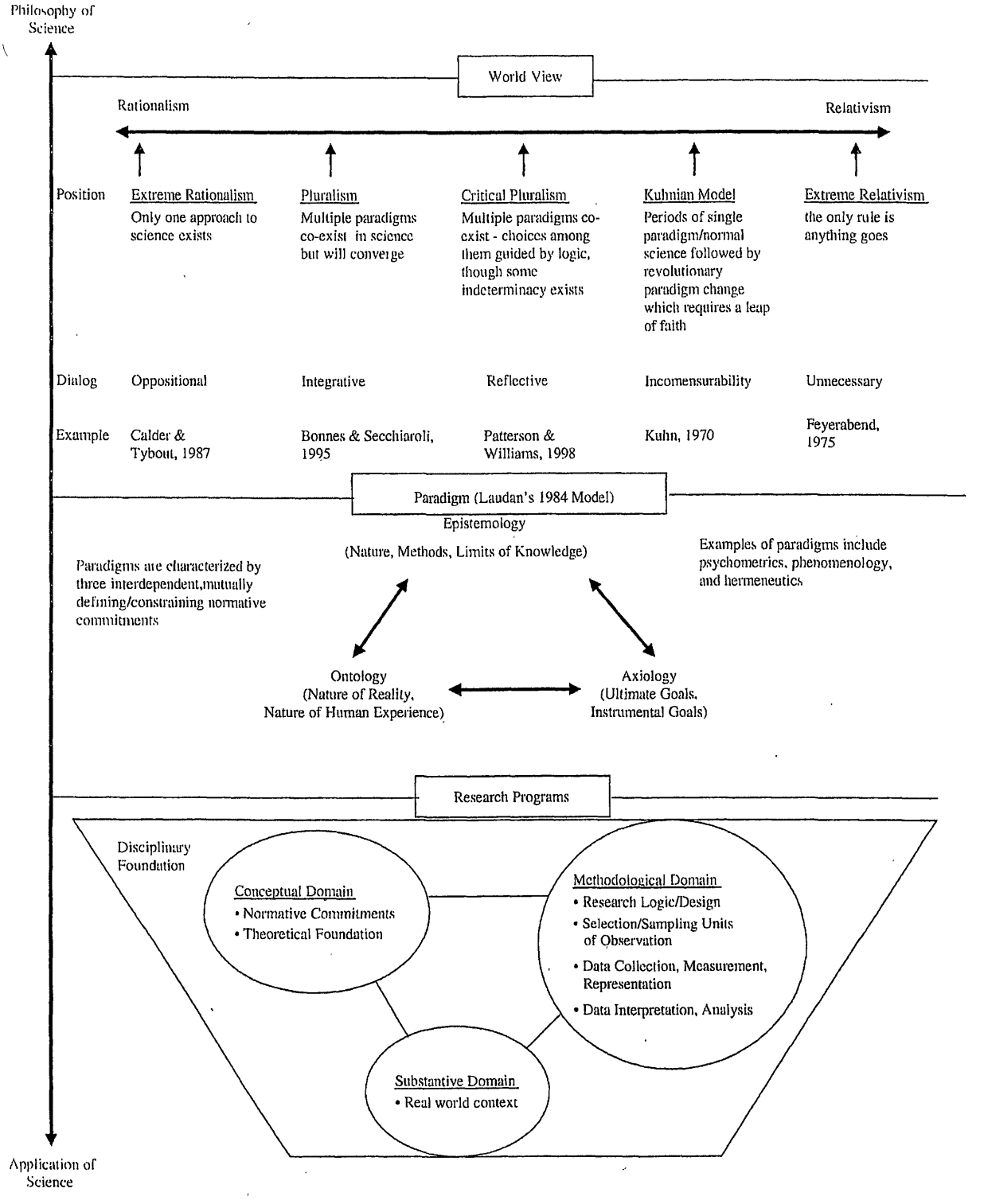

Fig. 1. Framework for mapping the epistemological foundation of research traditions (adapted from Patterson & Williams, 1998).

name and definition of the concept. Stedman (2003) also states that "the theoretical relationship between (various place) concepts remains poorly articulated; concepts often cannot be differentiated by their definitions" (p. 824). Further, he argues that Stage II in Low and Altman's (1992) trajectory of concept development has not been forthcoming (Stedman, 2002, 2003). Specifically, Stedman points to the absence of a systematic analysis of

relationships among concepts and the failure to develop a general theory of place.

In terms of the body of place research at the Research Program level as a whole, we argue that these critiques are overstated and do not reflect the degree of systematic theoretical development evident in the inter-disciplinary body of existing place-based research programs. At the Research Program level, we believe this overstatement arises as a result of: (a) too narrow a focus on the definition of specific concepts rather than taking a broader view examining the conceptual origins of different research programs, (b) failure to distinguish among distinct research programs arising from disparate theoretical research traditions, and (c) misconceiving of the body of place research as a single research tradition from which a single overarching theory of place could and should emerge.

Reviewers focusing on the historical development of specific research programs within the broad domain of place research have perceived a greater degree of coherence and conceptual clarity than the critiques discussed above seem to acknowledge. For example, focusing on the work of specific individuals, Gustafson (2001) develops a characterization of prior place research into which he integrates his study. Taking a broader disciplinary focus, Bonnes and Secchiaroli (1995) draw upon Saegert and Winkel's (1990) framework of research programs within environmental psychology to organize a discussion of the body of place-based research. In fact, Saegert and Winkel's organization integrates well with similar efforts in social psychology (Omodei & Wearing, 1990) as well as in more applied fields such as human dimensions of natural resource management (Williams & Patterson, 1999) and tourism (Mannell & Iso-Ahola, 1987).

In a similar effort based on a belief in the importance of placing individual studies in a larger conceptual whole, Seamon (1987, 1993, 2000) has taken a paradigmatic (defined in the sense of Fig. 1) rather than a disciplinary approach. His work seeks to organize place-based research programs originating from a cross-disciplinary, phenomenological perspective. A paradigmatic approach to organizing phenomenologically based place research is more consistent with the origins and evolution of that research tradition than is a disciplinary approach. This also reflects the diversity through which research programs can evolve.

Fig. 2 presents a framework that integrates these prior efforts to systematically organize place-related research programs on the basis of common themes and core underlying assumptions. This framework provides a useful structure for organizing a discussion of a diverse array of research programs. But, as with any attempt to organize a dynamic, interdisciplinary body of research, this framework does not represent the only way to organize different research programs, nor is it inclusive of all disciplines or all place-related research programs. Fig. 2 emphasizes research programs originating from social psychology and environmental psychology as well as the closely associated applied fields of consumer behavior and tourism and recreation research. A characterization of the body of place research emphasizing these disciplinary traditions has implications for what is and is not included. The social psychological tradition, for instance, has tended to conceive of relationship to place as a source of happiness and well-being. However, Manzo's (2003) recent review notes that relationship to place may be negative and that this aspect has been under-represented in place research. Additionally, a critical perspective in which place relationships are seen as being mediated by socially and historically constituted power relationships (Kincheloe & McLaren, 1994; Mitchell, 2003; Soja, 1989, 1996), has only recently begun to have an impact on nature-based place research (cf., Manzo, 2003; Williams, 2002). This research originates primarily from geography and sociology and is not reflected in Fig. 2.

While Fig. 2 is not all inclusive and represents only one of many possible ways of organizing place-based research programs, it does provide a basis for responding to critiques about absence of conceptual clarity and lack of systematic analysis of relationships among concepts reviewed above. At the Research Program level, place research traditions can be systematically characterized and differentiated on the basis of the deeper philosophical structure underlying research programs. This philosophical structure is comprised of assumptions about issues such as the nature of reality (e.g. single versus multiple), the nature of human experience (e.g. determinism versus situated freedom), and epistemology (e.g. multivariate versus holistic). The bottom half of Fig. 2 is an attempt to identify some of the key philosophical dimensions that distinguish among the set of place-related research programs identified.

These underlying distinctions are part of the basis for arguing that place research is not a research program in itself, but a broad domain of research informed by a multitude of interdisciplinary research programs each reflecting differing philosophical assumptions. Conceptual clarity comes from an understanding of the history of these research traditions and analysis of points of convergence and divergence. Unfortunately, these philosophical distinctions are often taken for granted and not explicitly acknowledged or recognized, particularly in second-generation research that seeks to empirically evaluate theoretical concepts (Raguraman, 1994).

Adopting a Research Program level viewpoint, rather than focusing more narrowly on the definition of specific place concepts (e.g. constructs such as place identity), makes it possible to fully consider the implications of recommendations intended to enhance conceptual clarity presented in recent critiques of place research. For example, consider Jorgensen and Stedman's suggestions that place research adopt "well established" and "relatively conventional" measures from the social psychological model (Stedman, 2002, p. 561; 2003, p. 827), that place-related constructs be regarded as attitudes (Jorgensen & Stedman, 2001), and that place satisfaction be considered

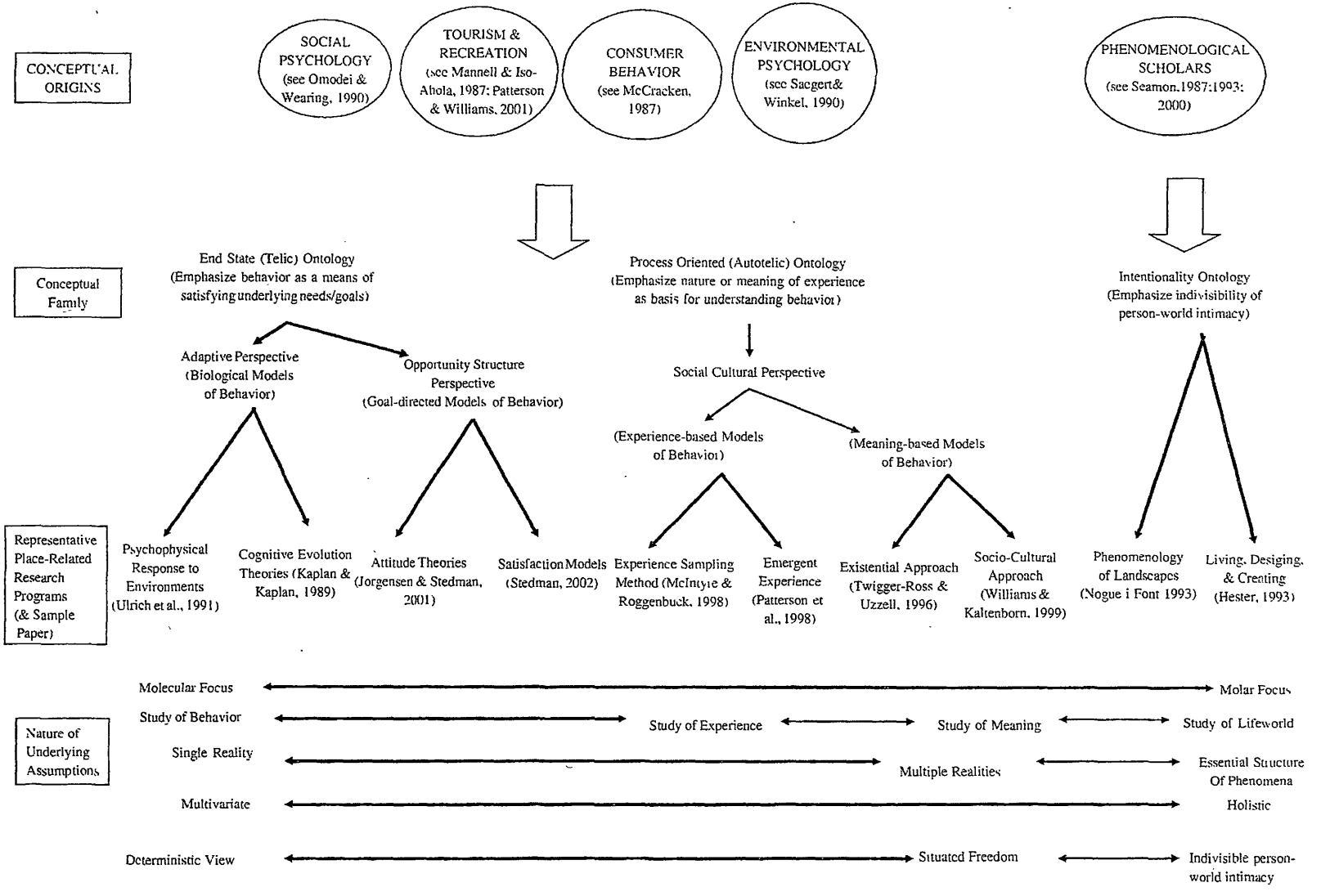

Fig. 2. A partial map of Research Programs on place organized according to research traditions (adapted from Patterson & Williams, 2001). (McIntyre & Roggenbuck, 1998; Nogue i Font, 1993; Twigger-Ross & Uzzell, 1996; Ulrich et al., 1990; Williams & Kaltenborn, 1999).

an integral aspect of place research (Stedman, 2002). As these authors suggest, attitude and satisfaction theories do reflect well-developed research programs within social psychology. Additionally, they provide an established framework for operationalizing constructs linked to affect, cognition, and behavior. However, underlying attitude and satisfaction theories are a set of assumptions about the nature of the phenomenon being studied, the unit of analysis, and human nature (Fig. 2). Specifically, attitude and satisfaction theories are located toward the molecular end of the spectrum, typically employ multivariate analytical techniques, and reflect a view of humans as rational analytic information processors. Further, in social psychology, attitude and satisfaction theories have been characterized as end-state or telic frameworks (Omodei & Wearing, 1990).

In contrast, much place research reflects a very different set of assumptions (tending toward the molar end of the spectrum, advocating holistic methodologies, and viewing humans as actively constructing meaning rather than processing information). These assumptions are characteristic of process-oriented (autotelic) frameworks (Fig. 2). In fact, though different authors use a variety of labels to express the differences depending on disciplinary and substantive backgrounds, research programs employing end-state frameworks and those employing process frameworks are usually presented in contrast and/or opposition to one another (cf., Mannell & Iso-Ahola, 1987; McCracken, 1987; Mick & Buhl, 1992; Omodei & Wearing, 1990; Patterson, Watson, Williams, & Roggenbuck, 1998; Saegert & Winkel, 1990).

The distinctions recognized between end-state and process-oriented research programs suggest careful consideration should accompany any suggestion that end-state models should be adopted as the basis for achieving conceptual clarity in the study of place. Differences in the assumptions underlying different research programs can have significant implications, leading to development of very different theoretical mechanisms as explanations for human behavior. For example, Patterson et al. (1998) discuss how approaching place-based research on recreation experiences from a process-oriented/emergent experience model versus an end-state/satisfaction model (see Fig. 2) can lead to very different views about the types of psychological mechanisms thought to underlie behavior and very different lines of inquiry. Thus, while it is not inappropriate to try and infuse new life and insight or greater clarity into the study of place by pursuing a research program that has not been the primary foundation for place research in the past, such attempts should be approached with caution. There is particularly a danger inherent in attempts to reduce diversity by standardizing terminology, attempts to insist on a single definition for concepts as abstract as the notion of place, or suggestions that an alternative research tradition is better suited as a basis for empirical study than research traditions that led to the original germination of a set of ideas. The result may be attempts to wed philosophically incompatible ideas. Similarities may be only superficial and attempts to integrate may actually contribute to miscommunication or misunderstanding by obscuring significant conceptual distinctions.

Recognizing that place is a broad domain of research in which concepts have developed across multiple disciplines and research programs has another implication relative to recent criticisms about the perceived lack of clarity that results from diversity in definition of specific place-related concepts. There will always be some degree of segregation across disciplines due both to differences in orientation and to the rapid proliferation of knowledge and information in any given discipline. Partly this arises because the generic term used to initiate discussions about the overall phenomenon evolves separately in different disciplines. For example, human geography and the design fields appear to have focused on the broad term "sense of place" to designate the general domain of research (cf., Hay, 1998; Relph, 1997; Shamai, 1991) whereas environmental psychologists have often favored "place attachment" as the covering term for the broad domain (cf., Altman & Low, 1992; Guiliani & Feldman, 1993). The latter term can be particularly confusing at times because place attachment also refers more narrowly to a specific aspect of the overall relationship to place (cf., Williams, Patterson, Roggenbuck, & Watson, 1992).

Cross-discipline variation also arises due to the fact that the more abstract and philosophical a concept such as place is, the more room there is for variable interpretation of the concept (and even of readings of the same text about that concept) (Raguraman, 1994). As Raguraman notes, the list of terms added to the vocabulary of a discipline is not only rapidly evolving, but also reflects increasingly difficult philosophical concepts and language. The opportunity for variable readings and interpretations of these ideas leads to a tendency to alter the original meaning of ideas through second-hand reinterpretations. Thus it is increasingly common to find researchers "talking past one another even though they claim to espouse the same philosophy" (p. 245). Additionally, Raguraman notes this problem may lead researchers to apply concepts inaccurately or in an inappropriate context.

However, while variation in use of terminology does increase the possibility for miscommunication and lack of clarity, when considering whether this represents a flaw in the literature that requires remediation, it is important to make a distinction between inconsistency in terminology within a paper or research program versus variation in the use of terminology across research programs and disciplines. Within an article or a particular research program, inconsistent or interchangeable use of terminology is an inherently redressable impediment to clarity in communication. And, with the exception of situations where ideas have changed, should be viewed as an undesirable situation. In contrast, some degree of variation in terminology across research traditions is an inevitable

reality of scholarship. Rather than indicating a failure in the literature, for research phenomena like place with the types of interdisciplinary and conceptual characteristics described above, a greater burden of responsibility shifts to those interested in the topic to be attentive to the history of different research traditions within the domain of interest. Literature analyses and syntheses that develop organizing frameworks characterizing points of convergence and divergence across a research domain such as place are a fundamental skill of scholarship.

2.2. Critiques of place research and epistemological traditions at the paradigmatic level

As noted above, different research programs reflect different philosophical assumptions. Many of these assumptions stem, not from the research programs themselves or the disciplines in which they are situated, but from the ontological, epistemological, and axiological commitments at the paradigmatic level (Fig. 1). Thus a thorough understanding of the nature and history of a research tradition also requires an understanding of the normative philosophical commitments of the paradigm that guides its empirical development. Often these philosophical commitments are taken for granted, especially in second-generation research. However, we argue that recent critiques expressing a concern for lack of conceptual clarity in the body of place research also are driven by views at the paradigmatic level of research traditions. Thus understanding and responding to these critiques requires consideration of this level in the macrostructure of research traditions.

Relative to research programs, paradigms are less directly concerned with the development of a specific theoretical concept and are more directly concerned with normative philosophical commitments (ontology, epistemology, and axiology) that serve as guidelines for the development of theoretical concepts in general (Patterson & Williams, 1998). Many paradigms (e.g. pyschometrics, phenomenology, hermeneutics, critical theory) have been employed in place research, but only two are considered in-depth here: psychometrics[1] and phenomenology. These two were selected because the recent critiques of place research noted above stem from the normative commitments underlying psychometrics and typically challenge or question the adequacy of phenomenological research. The following discussion explores how different philosophical commitments within these two paradigms lead to incompatible methodological directives. Additionally, it explores how paradigmatic commitments shape the meaning of terms, an issue that has helped stimulate recent critiques regarding lack of conceptual clarity in the domain of place research.

Many of the recent critiques of place research are directed at issues related to operationalization. For example, Lalli (1992) suggests that lack of empirical operationalization is one of two major factors contributing to what he perceives as insufficient research applications in the study of place. Stedman (2002) states there have been few attempts to build systematic theory in place research and attributes this in part to inconsistent measurement. He advocates adopting a social psychological model (attitude theory) in part due to a belief that it offers operational advantages including "clearer and more agreed upon" constructs for which "the relationships between variables are empirically specifiable" (p. 563). And Shamai (1991) states that the more accurate definitions of place concepts have been those in which "an operational definition was required for an empirical study" (p. 347). Each of these authors emphasize a need to infuse quantitative operationalization into empirical research on place to make possible the precise, rigorous, and systematic analyses demanded in science.

Though on the surface these may appear to be simply methodological critiques, they actually reflect deeper philosophical commitments underlying the psychometric paradigm. Psychometrics can be understood as a paradigm that arose in response to questions of whether it is possible to have a science that studies intangible social and psychological concepts such as preferences, emotions, and mental abilities. The psychometricians' response was that "we can study social and psychological phenomena scientifically ... (but) to do so it is necessary to measure" (Anderson, Basilevsky, & Hum, 1983, p. 233). However, their definition of measurement is limited to those empirical observations and analyses that involve quantitative representations of data. For example, Churchill (1979) defines measurement as the assignment of numerals to objects or events according to rules. While Anderson et al. (1983) define measurement as involving:

> [1] a theoretical domain, ... [2] [an] area of substantive concern reflected as an empirical relational system, ... [3] a domain represented by a particular selected numerical relational system, ... [and 4] a mapping function that carries us from the empirical system into the numerical system (p. 233).

Around this viewpoint, a complex structure of normative philosophical commitments has been built. For example, ontological notions about "true scores" are the basis for deriving mathematical expressions of validity and reliability (cf., Anderson et al., 1983; Churchill, 1979).

[1] The term "positivism" has often been used in discussions contrasting the research tradition labeled psychometrics in this paper with phenomenological research (Peet, 1999, pp. 24-25). We use the label "psychometric" because its greater specificity makes it a more apt description for a paradigm as conceived in this paper (i.e. an approach to science characterized by a mutually defining set of ontological, axiological, and epistemological commitments). Positivism is less adequate in this context because of its breadth when used in a general sense (e.g. behaviorism could be considered a positivist paradigm as well despite its epistemological differences with psychometrics). When used in a more specific, philosophy of science sense (i.e. logical positivism), this term is not adequate because contemporary quantitative approaches are strongly informed by post-positivist philosophies (e.g. Karl Popper) (cf., Chalmers, 1982).

Epistemological notions that observation is theory dependent and that it is never possible to conclusively establish that theory-based observations are true combined with a belief that it is possible to definitively demonstrate a hypothesis is false have led many adherents of this paradigm to adopt hypothesis testing in a falsificationist formulation[2] as the most appropriate way to test the logic linking theoretical concepts/conclusions to empirical observations. Statistical algorithms make possible prediction as a terminal axiological goal while at the same time allowing specification of instrumental goals (the basis by which the data from a specific study are judged as scientifically legitimate) such as acceptable p-values, r^2, eigenvalues, and reliability coefficients. Thus, within the psychometric paradigm, the underlying normative philosophical commitments require theoretical concepts with definitions that are narrow and precise enough to allow quantitative operationalization. This requirement leads adherents of this paradigm to see a lack of conceptual clarity in the absence of concepts with theoretical definitions of this nature, as illustrated in critiques of place research described above.

However, as noted previously, phenomenologists reject the very notion that place is a concept suited to a precise definition or that conceptual clarity can be achieved via quantitative operationalization of narrowly defined constructs (Relph, 1976; Seamon, 1987). The phenomenologists' perspective stems from normative philosophical commitments at the Paradigmatic level as well. However, before an explanation of the normative commitments underlying phenomenology can be provided, the specific phenomenological research tradition being described must be identified. At a broad level, phenomenology can refer to a family of interpretive paradigms including the philosophical phenomenology associated with Edmund Husserl and the existential phenomenology associated with Martin Heidegger and Maurice Merleau-Ponty (Giorgi, 1997; Seamon, 2000; von Eckartsberg, 1998). Some authors also include the hermeneutic tradition of Paul Ricoeur and Hans Georg Gadamer as part of the phenomenological tradition (Seamon, 2000). These paradigms are overlapping and share key similarities, especially when contrasted with the psychometric paradigm. However, from the concept of paradigms as conceived in this paper (a research tradition defined by a set of inter-connected ontological, epistemological, and axiological commitments), philosophical, existential, and hermeneutic phenomenology each represents a sufficiently different set of commitments to be considered distinct paradigms.

[2]Briefly, falsificationism describes a testing logic that maintains: (1) theories can never be proven true; (2) it is hypothetically possible to prove theories false empirically through deductive logic; (3) acceptable theories, therefore, must be falsifiable (there exist observations that have the logical possibility of being inconsistent with theory), and (4) science progresses by putting theories to increasingly critical tests that attempt to falsify them (cf., Chalmers, 1982).

The characterization of phenomenology presented below represents a paradigmatic research tradition stemming most directly from Husserl's philosophical phenomenology. A thorough understanding of the nature and implications of the normative commitments stemming from this paradigm requires an understanding of the phenomenological meaning of fundamental ontological concepts including phenomenon, consciousness, and intentionality. These are philosophically complex concepts, not readily explained or understood in terms of concise definitions. A brief overview of these concepts is provided below. A more thorough, and highly readable, explanation of the ontological, epistemological, and axiological aspects of this paradigm can be found in Giorgi (1997).

Within this phenomenological paradigm, the term 'phenomenon' means "the presence of any given precisely as it is given or experienced" (p. 237). Giorgi explains that this means this paradigm is concerned with presences (or objects) as they appear in consciousness. That is, objects are not of interest in terms of their 'objective,' 'real,' or 'existential' sense; rather the focus is on the meaning "of the object precisely as it is given" to an individual (p. 237). Giorgi provides the following example to illustrate the notion of 'givenness' as opposed to the real, objective, and existential nature of objects:

> Person A may view a painting and call it ugly, person B may view the same painting and call it beautiful. For person A, the painting will have all of the phenomenal properties of ugliness, and for person B, it will have the phenomenal properties of beauty. However, (from) a phenomenological perspective no claim is made that the painting is in itself either ugly or beautiful; only its presence for the experiencer counts, and an accurate description of the presence is the phenomenon, and it usually contains many phenomenal meanings (p. 237).

The consciousness of phenomenological ontology is conceived "not as a 'neutral' presenter of objects" but as something that "contributes to the very meaning of ... objects by its varying modes, styles, forms, and so forth" (Giorgi, 1997, p. 236). Phenomenologists following Husserl view intentionality as "the essential feature of consciousness" (p. 237). It signifies the notion that consciousness always has an object-consciousness is always consciousness of something (Giorgi, 1997; Valle, King, & Halling, 1989; von Eckartsberg, 1981; Wertz, 1989). Phenomenologists see this ontological perspective as overcoming the dualistic 'subject–object' dichotomy. From an intentional conception, the person and object are indivisible (Giorgi, 1997; Seamon, 2000).

These ontological commitments are one of the bases for phenomenologists' epistemological aversion to the type of operational measures employed in psychometrics. Phenomenologists maintain that the indivisible subject–object relationship described by intentionality must be understood structurally (as inter-relationships among elements) and holistically (Giorgi, 1997). Phenomenologists maintain

that this type of structural, holistic understanding cannot be accomplished through the types of concise operational definitions employed in psychometric epistemology.

Researchers associated with Duquesne University have been among the leaders in making a concerted effort to translate phenomenological normative commitments into concrete steps of a scientific method for advancing phenomenologically grounded research programs (Giorgi, 1997; Giorgi, Barton, & Maes, 1983; Seamon, 2000). For example, Giorgi (1997) states that to be genuinely considered phenomenological, research would have to be: "(1) descriptive, (2) within phenomenological reduction and (3) seek[ing] at least individuated meanings of some sort, and with the help of free imaginative variations, search[ing] for more invariant or essential meanings." As methodological guidelines for conducting phenomenological research, each of these terms can only be adequately understood in the context of phenomenology's paradigmatic normative commitments.

Giorgi (1997) provides a highly accessible explanation of the paradigmatic meaning of these terms. A descriptive approach is conceived as one that "limit(s) itself to what is given" "precisely as it appears within that act" of intentionality (p. 241). Often this description is obtained through interviews with respondents describing their experience from a natural attitude (an unreflective state where things are taken for granted). Phenomenological reduction is accomplished through analysis by the researcher. In this analysis, the researcher seeks to bracket or set aside past knowledge "in order to be fully present to the concrete instance of the phenomenon as presented by the subject's description" (p. 244). For example, Giorgi describes analysing an interview on learning during which he puts:

> aside all theories of learning as well as all personal experiences of learning, and simply contemplate(s) the description before me as belonging to the subject who wrote it. In addition, I will only assert that the description refers to how the subject construed the situation, and not that it was really the way he or she took it to be (p. 244).

Imaginative variation is an analytical exercise that seeks to identify the essence or essential structure of the phenomenon. It entails the free changing of

> aspects or parts of a phenomenon or object, ... (to see) if the phenomenon remains identifiable with the part changed or not. ... Whatever is given factually (in the description) becomes one example of a possible instance of the phenomenon, and by multiplying possibilities one becomes aware of those features that cannot be removed and thus what is essential for the object to be given to consciousness. (p. 243).

Though not a complete characterization of the paradigmatic commitments of psychometrics and phenomenology, the discussion presented above is hopefully sufficient to illustrate how the differing normative commitments underlying paradigms help explain why some place researchers see lack of conceptual clarity and inadequate theoretical development. It also helps to explain why some researchers see narrow and precise definitions of concepts as the route to clarity while others see this as a misguided and counterproductive venture. Beyond these operational implications, a failure to understand these underlying philosophical differences at the Paradigmatic level also contributes to the potential for confusion, miscommunication, and apparent lack of clarity in other ways.

Normative philosophical assumptions at the paradigmatic level give meaning to the very language used to express fundamental concerns within a research tradition. The phenomenological notion of "consciousness" described above is one example. Similarly, the phenomenological notion of a 'descriptive' approach discussed above represents another example. And ultimately, different paradigms may ascribe different meanings to the same term. An example relevant to place research is different interpretations of the term "particularlistic." Adherents of both psychometrics and phenomenology have criticized research grounded in the other paradigm as being too particularistic. From a psychometric perspective, Stedman (1999, 2002, 2003, p. 827) characterizes phenomenological approaches as "radically particularlistic" and attributes failure to build a systematic knowledge base or derive a general set of principles in place research in part to this aspect of phenomenological paradigms. Similarly, Gieryn (2000) suggests that apparent lack of interest in place as a theoretical construct in sociology may be due to a fear "that the particularities of discrete places might compromise the generalizing and abstracting ambitions of the discipline" (p. 464). In contrast, Peet (1999) noted that phenomenologists consider the view of scientific knowledge characteristic of the psychometric approach to be "blind ... to most forms of human experience" because of its "narrow-minded, highly particularistic" view (p. 48).

That phenomenologists characterize the psychometric tradition as overly particularistic while the psychometric tradition describes phenomenology as overly particularistic does not necessarily mean that one or the other is using the term particularistic incorrectly or is mischaracterizing the other paradigm. Within each paradigm, the particularlistic critique stems from their underlying normative commitments. The particularistic critique in the phenomenological sense stems from ontological issues. Specifically, it refers to the tendency for psychometrics to adopt a "molecular" approach which views phenomena as capable of being reduced to a set of interacting elements or variables, rather than a molar approach that conceives of phenomena more holistically as transactional dimensions whose whole is more than the sum of its parts (Altman & Rogoff, 1987; Bonnes & Secchiaroli, 1995; Seamon, 1987). Thus, attempts to reduce phenomena like place to the kind of precise, narrowly defined constructs of the sort required by the psychometric paradigm are viewed as overly

particularistic. Yet this viewpoint challenges the very premises on which psychometric epistemology is based.

In contrast, the psychometric use of the term "particularistic" stems from axiological concerns. Within psychometrics, the ultimate aim of science is to produce general or universal laws. Case studies of individuals, single communities, or unique places are seen as an inadequate basis for achieving this type of knowledge. Because phenomenology employs these types of epistemological strategies, those grounded in the psychometric paradigm view the body of phenomenological research as overly particularistic. However, from the viewpoint of the phenomenological paradigm, the charge of particularism raised by those with a psychometric viewpoint is interpreted as a challenge to, or a misunderstanding of, the underlying nature of consciousness (described above). This assumption is linked to ontological assumptions about the nature of the phenomenon being studied (phenomenological meaning rather than objective information inherent to the stimulus), epistemological assumptions about the appropriate unit of analysis, etc.

The use of the term particularistic is not the only example of one for which there are dramatic paradigmatic differences. A similar analysis could be applied to the notion of "reduction" across the two paradigms. While the differential use of concepts such as particularistic, reduction, and other terms across paradigms complicates communication and can contribute to a perception of lack of conceptual clarity within the body of place research, multiple uses can reflect appropriate critiques from the perspective of their paradigm's underlying normative commitments. In such a situation, conceptual clarity cannot be attained by insisting on standardized terminology. Individual terms cannot adequately convey the complexity of the system of paradigmatic assumptions that underlie a research tradition. Clarity can only come from understanding the paradigmatic context in which the term is being used in conjunction with an understanding of the normative assumptions underlying the relevant paradigm. Thus, in the evolution and progression of a domain of research like place, once erosion of initially presumed consensus is reached, it is our belief that Low and Altman's (1992) Stage III does not mean standardization across the entire domain of research, elimination of diversity in approach, or development of a single overarching theory. Rather, progression means just what Low and Altman stated—"there is development of systematic theoretical *positions* and clearly delineated *programs* of research" (p. 3, emphasis added). However, our perspective here reflects a position at the World View level of research traditions.

2.3. Critiques of place research and epistemological traditions at the World View level

The preceding analysis of research traditions from the paradigmatic level reveals that incompatible epistemological directives may exist across paradigms (as illustrated above for psychometrics and phenomenology). Within a paradigm, questions about validity, what counts as evidence, what represents a legitimate epistemological application, etc. can be resolved through an appeal to its underlying normative commitments. However, the question of how to resolve differences in perspective on these types of issues across paradigms is a matter of substantial debate dealt with at the World View level.

One way of characterizing world views is on the basis of the position that they reflect along a continuum that ranges from extreme rationalism to extreme relativism (Fig. 1) (Patterson & Williams, 2001). These positions are identified on the basis of ideological underpinnings that shape how researchers respond to diversity in approach at the Paradigmatic level. Extreme rationalists hold the viewpoint that there is one and only one approach to science. Often this approach is presented as a set of epistemological rules for the conduct of science referred to as "*the* scientific method". An extreme rationalist perspective is evident in Calder and Tybout's (1987) assertion that the body of scientific knowledge only consists of research conducted in compliance with the principles of falsificationism and that research conducted from interpretivist paradigms (such as phenomenology) is merely entertaining reading that must stand apart from science. At the other end of the continuum is extreme relativism that maintains that no rules of science can ever be specified. Representative of that extreme is Paul Feyerabend's (1975, p. 296) assertion that "(a)ll methodologies have their limitations and the only 'rule' is 'anything goes'."

Thomas Kuhn's (1970) position represents a point between extreme rationalism and extreme relativism. Kuhn's belief that there are periods of normal science in which the conduct of science adheres to a single paradigm that sets the standards of legitimacy for scientific research reflects a rationalist dimension. However, according to Kuhn, crises in the accepted paradigm eventually lead to the emergence of a new paradigm during a period of revolution. Adoption of the new paradigm in Kuhn's model requires something akin to a religious conversion because no purely logical argument demonstrating the superiority of one scientific paradigm over another can be made (Chalmers, 1982). This reflects a relativist dimension to his world view.

A second way of characterizing world views is according to the type of dialog they generate when confronted with paradigmatic differences in normative commitments of the sort illustrated in the preceding section. These dialogs describe the form and nature of response different world view positions adopt in regard to questions of legitimacy, validity and diversity arising from across-paradigm ideological differences. Franck (1987) suggested that responses to paradigmatic diversity can broadly be thought of as falling into three classes of dialog: oppositional, integrative, and reflective. Though this grouping does not include a Kuhnian dialog centered around incommensurability or the nature of dialog under extreme relativism, the three

forms of dialog identified by Franck appear to encompass the range of dialogs evident in body of place research. The discussion below describes the nature of different world view positions (rationalism, pluralism, and critical pluralism), the types of dialogs they generate about paradigmatic diversity (oppositional, integrative, and reflective), and their relevance to understanding and responding to recent critiques about clarity and progression in the body of place research.

2.3.1. Rationalist positions and oppositional dialogs in place research

Many recent critiques questioning the conceptual clarity, coherence, and adequacy of the existing body of place research reflect a strong rationalist viewpoint. Stedman (2003, p. 824), for example, attributes "lack of construct clarity, and the dearth of attempts to better systematize relationships between constructs .... [to the historical prominence of] phenomenological approaches ... [which] use qualitative methods and reject the language of conventional positivistic science that [in contrast to phenomenology] emphasizes hypothesis testing and prediction via general laws of human behavior." Jorgensen and Stedman (2001) further critique earlier place theorists such as Relph and Tuan for "either explicitly identify[ing] place research as a phenomenological endeavor or otherwise ... not us[ing] empirical methods to 'test hypotheses' in any formal sense" (p. 234). Similarly, responding to Lewis's (1979, p. 40) statement that it is "quite useless" to try and measure sense of place, Shamai (1991) responds that "it is impossible to measure only if one holds a specific philosophical point of view (i.e. phenomenology) that regards it as impossible to quantify any phenomenon" (pp. 354–355). While Shamai does express the view that "the process of sense of place" cannot be quantitatively measured, consistent with a rationalist psychometric World View, he also expresses the view that "the process of sense of place" (and other phenomena that cannot be quantified) "is beyond the scope of ... empirical study" (p. 354).

Though not a mandatory requisite of the psychometric paradigm, its adherents often come to hold a rationalistic world view like those just presented, largely for epistemological reasons (the normative commitments related to the nature, methods, and limits of knowledge). Specifically they come to believe it is not possible to study relationships scientifically in the absence of numerical measurement and quantitative analyses. For example, Anderson et al. (1983, p. 233) state that "the problem of establishing functional relationships involving many variables probably cannot even be stated clearly, much less solved, without the tools of traditional mathematical analysis." This underlying world view may help explain Stedman's (2002) equating attitude theory, not with a research program within social psychology, but as *the* social psychological model. Thus Stedman and Jorgensen's suggestion that this model be adopted as the basis for achieving conceptual clarity and building systematic knowledge in place research (Jorgensen & Stedman, 2001; Stedman, 2002, 2003) appears to reflect more than just a commitment to a research program (attitude theories within social psychology) or a paradigm (psychometrics). It also reflects a commitment to a rationalist world view that conceives of psychometrics as the scientific model and hypothesis testing as the only scientifically meaningful testing logic.

Rationalist world views give rise to oppositional dialog as reflected in Stedman's (Jorgensen & Stedman, 2001; Stedman, 2002, 2003) suggestion that lack of conceptual clarity and progression in place research stems from its phenomenological foundations. At times, the rationalist view may be expressed in a milder form as reflected in the following statements about phenomenological research:

> [Phenomenologists] take a particularistic view of sense of place and eschew deriving generalizations from hypothesis testing. This approach has much to recommend; it provides details and intimate knowledge about how place works in a given setting for a given group of actors. Such an approach may impede the development of general principles that can be examined across settings. Also, this approach may be a barrier to integrating place variables with traditional forest management, which has relied more heavily on conventional positivistic science and its hypothesis testing approach (Stedman, 2003, p. 824).

However, rather than embracing the legitimacy or recognizing the contribution of phenomenology, the manner in which this statement is presented suggests more of a grudging acknowledgement that an alternative, though empirically inadequate, paradigmatic tradition exists.

Rationalist world views are not limited to adherents of the psychometric paradigm, advocates of phenomenology can hold a rationalist perspective as well. In place research, phenomenologists have tended to express the milder forms of rationalism. For example, Relph (1970, p. 190) states that "from the basis of ... phenomenological assumptions, attempts to develop mathematical models and theories of ... behavior ... are seen not as a contribution to the understanding of some real geography of man's activities, but as the reflection of the limited intentions of those geographers presenting the explanations." As noted by Peet (1999), Relph further suggests that if geography is concerned with developing objective laws and theories, phenomenological critiques can be ignored but that if geography is concerned with understanding people on the human level, the concepts of phenomenology have much to offer. Peet (1999, p. 48) also notes a tendency among phenomenological geographers to characterize research conducted in line with the psychometric tradition as yielding a "pale, insipid understanding (that) is blind to many, if not most forms of human experience."

The type of oppositional dialogs rationalistic world views tend to generate across paradigms often are counterproductive because they entail the use of straw man tactics where misleading caricatures imminently suited for

demolishing are built. For example Calder and Tybout's (1987) caricature of interpretivist research (such as phenomenology) describes it as entailing the selective use of data to show how a conceptualization fits the data with no intention of comparing interpretations. This implies the complete absence of a testing logic (a logic by which the theoretical concepts are evaluated in light of empirical observations) and is a straw man portrayal of phenomenological research (see, e.g. Giorgi's (1997) description of the standard for critical evaluation underlying a phenomenological logic of analysis).

Another, more subtle, example of oppositional caricatures is reflected in Stedman and Jorgensen's statements that phenomenological place researchers "eschew deriving generalizations from hypothesis testing ... imped[ing] the development of general principles" (Stedman, 2003, p. 824), that phenomenologists make "strong" general statements about sense of place despite the absence of "empirical methods to test hypotheses in any formal sense" (Jorgensen & Stedman, 2001, p. 234), and similar assertions (Stedman, 1999, 2002). These statements are readily misleading. They are easily interpreted as implying that phenomenology does not seek to make general statements about phenomena or that it does not have an underlying testing logic. However, as Seamon (2000, p. 160) states, one of the ultimate aims of phenomenology is to "use ... specific instance[s] for identifying deeper, more generalizable patterns, structures, and meanings." Similarly, in defining a scientific phenomenological method, Giorgi (1997) identified the ability to produce general knowledge (application beyond just the situation studied) as a necessary scientific standard.

Oppositional assertions of the sort just illustrated divert the discussion away from the real issues. As Peet (1999) notes, all research involves generalization. And all research involves an underlying testing logic that links empirical observations to interpretations (Patterson & Williams, 2001). What differs, then, across paradigms is the underlying logic of generalization and the nature of the testing logic. It is the nature and implications of these latter differences, rather than oppositional caricatures, that merit consideration in a discussion of diversity among research traditions in a multi-tradition domain of research like place.

Further, a rationalistic insistence that one research tradition represents the only acceptable logic of analysis in the study of phenomena as complex as place seems to be an untenable position. As Peet (1999) notes, all paradigms entail abstraction from empirical particulars, employing processes of simplification, generalization, and essentializing that divorce an empirical phenomenon from its real-world counter-part. In other words, empirical analyses are simplified models of reality rather than reality itself. Research designs generating these simplifications are guided by philosophical assumptions that lead to defensible but not indisputable judgments regarding tradeoffs among competing threats to validity.

While the perspective that research designs simplify reality through decisions involving tradeoffs about competing threats to validity for which there are not indisputably correct choices may at first appear to be a radically relativist conception of science, it is not inconsistent with contemporary views underlying quantitative and hypothesis testing logics of analysis. Even the most advanced and widely endorsed hypothesis testing logic, falsificationism, takes the position that it is never possible to conclusively show something is true in science and that observation is theory dependent (thus there are no neutral facts) (Anderson et al., 1983; Chalmers, 1982). And in a recent discussion of structural equation modeling, MacCallum and Austin (2000, p. 218) state:

> With respect to model fit, researchers do not seem adequately sensitive to the fundamental reality that there is no true model ..., that all models are wrong to some degree, even in the population, ... Given this perspective, it is clear that a finding of good fit does not imply that a model is correct or true, but only plausible.

Phenomenologists recognize parallel issues. For example, Giorgi (1997) characterizes phenomenology's goal as being one of phenomenological reduction to identify the essential structure of human experience. In describing bracketing, Giorgi acknowledges that despite the practice of "bracketing" knowledge, scientific phenomenological analyses are influenced by different disciplinary sensitivities. Additionally he characterizes phenomenology as a specific approach to research that falls within the broader umbrella of science.

The discussion above illustrates that a more pluralistic (as opposed to strictly rationalistic) world view position is not inherently inconsistent with either a psychometric or phenomenological paradigmatic orientation. Contemporary understandings of science from both perspectives acknowledge a degree of empirical indeterminancy in individual studies. Thus, as Raguraman (1994, p. 246) notes, in such cases:

> the question of philosophical adequacy [of a given paradigm] cannot be answered a priori. Only with considerable amount of experience will one know if a path followed is a worthwhile one. Until that point it would be a good idea to 'exercise a degree of humility in the quest for truth' (Wallace, 1989, p. 3).

2.3.2. Pluralist positions and integrative dialogs in place research

A rationalistic world view/oppositional dialog is not the only world view position/dialog reflected in place research. A different world view position underlies Bonnes and Secchiaroli's (1995) discussion of place. While noting differences in paradigms and research programs underlying place research, Bonnes and Secchiaroli view them as illustrating "several directions environmental psychology is taking in order to construct a theory of place [that is]

able to give greater homogeneity and theoretical consistency to the field" (p. 193). They acknowledge the coexistence of different research traditions (both at the Paradigmatic and Research Program levels) yet make frequent references to the idea of integration and "development of a theory of place." Thus, these authors appear to reflect a pluralist world view engaged in an integrative dialog (Fig. 1). A similar notion seems to underlie Kaltenborn's (1998) critique of place research. Specifically, he states that while "(c)onstructing an empirical scale measuring sense of place using quantitative methods may appear to violate the nature of the concept[, t]he problem may be more philosophical than methodological" (p. 187). The perspective that philosophical differences may not have methodological or practical consequences, helps promote an integrative rather than oppositional stance toward different paradigmatic traditions.

A pluralist position/integrative dialog may be founded in part on a recognition that all paradigmatic traditions entail a similar broad logic of analysis in that "empirical particulars" are used to construct "general abstractions" (Peet, 1999, p. 3). In the case of place research, this viewpoint may also be combined with the tendency for ontologically appealing concepts from one paradigmatic tradition to be incorporated into other paradigmatic traditions without accompanying changes in epistemology. For example, Bonnes and Secchiaroli (1995) note that while it has been popular in environmental psychology to affirm a transactional–contextual perspective, "in reality this agreement ... [has been] followed more in programmatic intentions than in research practice" (p. 152). Such tendencies may give rise to the perspective that differences across research traditions are more philosophical than methodological or practical as expressed by Kaltenborn's statement above.

A world-view dialog that seeks integration rather than opposition among divergent paradigmatic traditions has the desirable quality of not seeking to undermine the relevance of whole bodies of research. However, it has problematic aspects as well. First, it is based on the assumption, reflected in the statements above, that philosophical differences do not have practical implications. In contrast, we maintain that Harvey (1969, p. 482) was correct when he stated that "philosophy provides the steering mechanism" for the use and application of methods within a research tradition. In other words, properly understood, philosophy does have ramifications for empirical practice.

For example, consider interviewing as a data collection method. One way to approach interviewing is from a stimulus response model (Mishler, 1986). This model treats the interviewer's questions "as a standard research stimulus ... [that is] expected to remain constant so that any variance in the response can be attributed to factors in the interview population" (Polkinghorne, 1988, pp. 176–177). Underlying this model is an objectivist ontology that maintains there is a "free-standing reality" (Howard, 1989, p. 187) and that knowledge is "a substance located in the minds, bodies, or personal experiences of others" (Nespor & Barylske, 1991, p. 806). In contrast, interviews may be conducted as directed conversations in which the researcher and respondent participate in an emergent discourse utilizing an interview guide and spontaneous probes (Patterson & Williams, 2001). This approach conflicts with several key aspects of the "stimulus–response" model (e.g. that each respondent must be asked the same questions in the same way) and reflects a very different set of ontological and epistemological assumptions (Charmaz, 1991; Patterson & Williams, 2001). It also requires a different analytic strategy—analysis of interviews collected under the "directed conversation" strategy cannot begin at the aggregate level (as is the case with statistical analyses such as *t*-tests, regression, etc.). Rather, if an aggregate-level analysis is to be conducted, an idiographic phase of analysis must occur first to structure the data in a way that permits an aggregate analysis.

On a broader scale, another difficulty with an integrative world-view dialog is that failing to integrate methodological practice with philosophical commitments can lead to failure to achieve the desired goals within a field of study. For example Malm (1993) discusses a perceived dilemma within cognitive psychology stemming from the failure to critically examine and change epistemological commitments related to methodological practice when ontological commitments within the discipline changed. As a consequence, with respect to the possibility of an integrative world-view dialog in the domain of place research, we believe that Franck (1987) was correct in asserting that the underlying assumptions and respective goals of research traditions as distinct as phenomenology and psychometrics are "so different ... that what one would achieve from some integration would be a strategy still based primarily in one perspective or the other" (p. 60) rather than a true integration.

A final problem with integration and standardization at the expense of diversity in paradigmatic approach across a domain of study like place logically stems from many of the same concerns associated with extreme rationalism/oppositional dialog. Research design requires tradeoffs among fundamental tensions as discussed earlier, and there is no definitively correct choice due to the fact that research goals may conflict with one another and threats to validity may be weighted differently (Kuhn, 1977; Mishler, 1990). Additionally it is possible for two researchers to see different (and not necessarily contradictory) general structures in the same set of facts (Peet, 1999) or for different structural models to fit the observed data equally well (MacCallum & Austin, 2000). In such circumstances, diversity in approach and perspective is a strength. If different approaches triangulate on the same conclusion, then we have reason for greater conviction. However, triangulation is only one of the possible outcomes of employing different research traditions. Other possible outcomes include: synergistically complementary findings,

entirely distinct but compatible insights, and competing or contradictory understandings. Thus the pursuit of integration carries substantial risk for the prospect of scientific progress.

2.3.3. Critical pluralist positions and reflective dialogs in place research

Accepting pluralism does not necessarily imply an integrative dialog. For example, Lalli's (1992) suggestion that some dimensions of place concepts can be quantitatively operationalized while others (e.g. the content of urban related identity) are more suitably explored through qualitative approaches seems to embrace a commitment to a pluralism without requiring integration. However, Lalli does not develop this perspective in more detail. In contrast, Franck (1987) goes into more depth in her call for adopting a reflective dialog in the face of incompatible paradigms. Her concept of reflective dialog entails a conversation where the goal is not supremacy of one research tradition or the synthesis of different traditions into a single tradition, but clarification and enrichment through an increased understanding and appreciation for the nature, benefits, and limitations of different traditions. Essentially, this reflects the critical pluralist world view (Patterson & Williams, 2001).

A critical pluralist world view differs from a pluralist position in that it does not strive for integration. At the same time, it does not equate with extreme relativism due to two stances underlying this view of science. First, this world view maintains there are criteria that distinguish science from other forms of knowing. For example, Patterson and Williams (2001) suggest there are three defining characteristics of science. The first is that science is empirical. This standard is meant to convey the ideas that science is grounded in observation, observations function as a test of theoretical concepts, and a testing logic explaining principles linking empirical observations to theoretical concepts is evident. The second defining characteristic is that the adequacy of the empirical test is subject to external criticism. This standard is meant to convey the notion that the presentation of the underlying conceptual framework, research logic, methods, and data must be sufficiently transparent to allow a relatively independent assessment of the warrants for a researcher's interpretations. The third defining characteristic of science is that it is systematic and rigorous. This means that scientific analysis does not entail selective use of data for the purpose of supporting preconceived ideas and that scientific analysis entails more than a cursory look at preconceived ideas; research is guided by a well-developed theoretical framework, set of research principles, and a detailed and defensible research design. As Patterson and Williams note, these defining characteristics of science are not incompatible with the nature of paradigmatic commitments in paradigms as diverse as psychometrics and phenomenology. For example, Giorgi (1997, p. 249) presents four characteristics that phenomenological research must meet in order to be considered "genuinely scientific" (systematic, methodological, general, and critical). Collectively, Giorgi's explanation of these four criteria yield a characterization of science highly parallel to characterization of science in Patterson and Williams (2001).

The second-stance critical pluralists adopt that distinguishes it from extreme relativism is its position that "nonevaluational, nonjudgmental, noncritical, or mindless pluralism" is an unreasonable stance (Hunt, 1991, p. 41). Critical pluralists argue that a logical choice among paradigms can be made on several bases including: the internal consistency of a paradigm's normative commitments; the attainability of a paradigm's goals given current methodological capability (Anderson, 1986); the fit between the paradigmatic assumptions (as expressed in the paradigm's normative commitments) and the researcher's assumptions about the phenomenon being studied; and the nature of research questions being asked. Additionally, critical pluralism also recognizes that scientific research is guided by normative commitments which differ across paradigms (Patterson & Williams, 2001). Research conducted within a paradigm is expected to adhere to, and be evaluated on the basis of, those normative commitments. For example, in addition to identifying criteria research must meet to be considered science, Giorgi (1997, p. 249) also identifies criteria scientific research must meet to be recognized as "genuinely phenomenological." And Pickles (1985) debates whether some research applications in geography that claim to be phenomenology actually is phenomenological in nature. Thus, critical pluralism reflects a more rationalist position than Kuhn's characterization of choice among paradigms as requiring a religious conversion or leap of faith.

However, critical pluralism does recognize a certain degree of indeterminacy in choice of paradigms with regard to a particular phenomenon. This indeterminacy stems from the perspective that a single set of methodological procedures cannot assure validity because validity assessments are based on judgments of the importance of different research goals and threats to validity. Critical pluralists believe that because research goals may conflict with one another and threats to validity may be weighted differently, different judgments about the acceptability of the necessary tradeoffs are possible and no single algorithm or set of standardized rules for assuring the best interpretation can be defined (Kuhn, 1977; Mishler, 1990). Research design, then, is thought of as an exercise requiring tradeoffs regarding competing threats to validity (fundamental tensions). For example, Brinberg and Hirschman (1986) refer to a tension between rigor (precision and control of variables and treatments) versus relevance (studying a phenomenon as it really exists—in a real-world context with all the elements present). And any research design reflects decisions across a multitude of fundamental tensions rather than on a single issue (Patterson & Williams, 2001). This stance, along with the view that

multiple paradigms legitimately co-exist within the broad realm of science, reflect the relativist aspect of critical pluralism as a world view.

Because of its pluralist/relativist and critical/rationalist dimensions, critical pluralism promotes a reflective dialog which seeks to explore and understand the differences in approach and insights across divergent paradigms rather than exclusion or integration. The concept of reflective dialog, is not, in fact a radical suggestion within science. It is one of the fundamental premises on which peer review is built. In science, academic practices such as presenting papers, entering debates, and peer review of research are "not an incidental condition of inquiry; ... [but] the very life of inquiry, discovery, and truth itself" (Wachterhauser, 1986, p. 33). This type of reflective dialog represents a living conversation characterized by an openness to the phenomena being researched (Bernstein, 1986; Wachterhauser, 1986). Ideally, peer reviewers see themselves as being engaged in a dialog devoted to helping develop an understanding of the issue, rather than as defending a position or serving merely as gate keepers for scientific accreditation (Wachterhauser, 1986). Such dialog can "bring the subject to life" yielding new insights and frameworks "that may suggest new ways of seeing the subject matter or new conceptual vocabularies ... [that can] help move a discussion onto new ground" (Wachterhauser, 1986, p. 33).

3. Conclusions

Numerous critiques of place research over the last decade have raised questions about the adequacy of past place research, focusing specifically on issues such as conceptual clarity, coherence, and theoretical progress. More generally, these critiques raise fundamental epistemological questions. How does science progress? How does one evaluate progress in the development of theoretical concepts? Is diversity in perspective and approach an indication of lack of conceptual clarity? Is one epistemology arguably superior to another? Is integration across perspectives possible? Are standardization and integration desirable?

Low and Altman's (1992) summary of the evolutionary trajectory of social science concepts emphasizing three stages provides a useful basis for considering the question of whether place research has progressed. By the late 1990s, erosion of consensus was clearly evident in numerous articles citing concerns about conceptual clarity and empirical adequacy, a situation that initiates Stage II in Low and Altman's proposed trajectory. On the basis of these critiques, Stedman (2002, 2003) suggests that place research has failed thus far to show much progress through Low and Altman's Stage II. In contrast, we believe this assessment is the result of viewing place research as if it represents a single research program. We argue that the body of place research is more appropriately viewed as a domain of research informed by multiple research traditions.

This distinction is an important one. How one should view the concepts of coherence and clarity differs whether one is looking within or across research traditions. Within a research tradition, the type of consistency and coherence called for in recent critiques is a precondition for progress. However, coherence in a domain of research entailing multiple traditions requires a fundamentally different viewpoint. Social science cannot be, or at least to this point has not been, reduced down to a common set assumptions as the research programs illustrated in Fig. 2 and the existence of paradigms as diverse as psychometrics and phenomenology illustrate. When comparing research programs, some philosophical assumptions are so incompatible that they cannot be wedded into a single operational approach. Thus, attempts to integrate across divergent assumptions may actually contribute to miscommunication or misunderstanding by obscuring significant conceptual distinctions in a way that ultimately leads to adverse consequences in the ability to achieve the goals of a research initiative (for an in-depth illustration, see Malm, 1993).

A rationalist-like oppositional dialog arguing for the superiority of one research program or paradigm is also problematic. First, a point of agreement among all contemporary philosophies of science is that science never provides absolute certainty that research has revealed the truth. In fact, one of the hallmarks of science is its capacity to evolve in the face of new empirical evidence. Divergent paradigmatic traditions play an important role in the progress of science understood from this perspective. If different approaches triangulate on the same conclusion, then we have reason for greater conviction. However, there are other possible outcomes including synergistically complementary findings, entirely distinct but compatible insights, and competing or contradictory understandings. Scientific progress thus benefits from paradigmatic diversity.

Understanding coherence and achieving conceptual clarity in Stage III place research ultimately requires frameworks and language that transcend research program, disciplinary, and even paradigmatic boundaries. We believe this is what Low and Altman (1992) had in mind when they stated that explicit attention is paid to definitions and to characterizing overtly the nature of different research traditions following erosion of consensus. And contemporary analyses from the philosophy of science seeking to describe the "macrostructure" of research traditions provide the basis for developing a transdisciplinary framework to organize this type of dialog.

Any such framework risks reifying what are, in fact, dynamic and evolving ways of thinking. And no single discussion is likely to be wholly comprehensive for a domain of research like place given the diverse set of research traditions and disciplines from which it has been explored. However, with these limitations in mind, a

framework is useful if it helps to reveal greater coherence in the body of literature, organize discussions about controversies and the state of knowledge, and guide practice. The framework presented in Fig. 1, which conceives of research traditions as being comprised of different levels of practice (Research Programs, Paradigms, and World Views), contributes to these goals. It creates a basis for understanding the historical coherence and systematic development of place research as well as provides a basis for analysing the origins and implications of recent critiques.

However, in this paper we have gone beyond merely introducing a framework to guide discussion. We have also advocated a specific normative position at the World View level, calling for researchers to adopt a critical pluralist perspective and a reflective dialog. But we see critical pluralism as only minimally prescriptive. It does not require researchers to abandon or change their normal (preferred) paradigm (a revolution in the Kuhnian sense of the term). It does require, however, an attitude of openness to, and appreciation for, other paradigms. It is possible for a researcher to adopt a particular paradigm and even to concentrate on it over the course of a career based on her/his belief that alternative paradigms are not as relevant/well suited without also having to characterize other paradigmatic approaches as inherently defective. An attitude of openness to alternative paradigms simply requires a researcher to recognize that the world is undisciplined and multifaceted; that all abstractions and models of it are, to some extent, limited and imperfect representations; and that, at some level of abstraction, it is always the case that relevance is in the eye of the beholder. This does not require the suspension of critical thinking, though it does require that once a research logic is adopted, critiques about its implementation be made in manner consistent with the assumptions underlying the adopted logic. As noted previously, this view of the relationship between the world and its representation through research is consistent with contemporary adherents of paradigms as diverse as phenomenology and psychometrics (cf., Giorgi, 2002; MacCallum & Austin, 2000).

We recognize that this call for critical pluralism runs counter to the time-honored preference in scientific institutions that promotes specialization and thus a narrowing of focus to a particular area and approach to research. Without disaffirming the benefits and necessity of specialization, in view of the relationship between the world and its representation just described, critical pluralism requires a broadening of focus to some degree. Further, we argue that merely appreciating or tolerating diversity is not sufficient. If all representations are limited and imperfect, if one is interested in helping construct as comprehensive an understanding of social phenomena as possible, and if coherence within a domain of research is to be achieved and maintained, then it is important for individual researchers to maintain some understanding of how their work fits into the larger domain of research on a particular topic rather than becoming isolated in a particular research tradition. Such an understanding does not require achieving a mastery of all the possible alternatives—something we believe would be impossible given the existing complexity and diversity of research traditions—but it does require an awareness of core distinctions and contributions from alternative approaches. Without such an understanding, generated through reflective dialog, diversity contributes to lack of clarity and is counter-productive rather than healthy.

Having argued for the value of diversity in paradigmatic approaches, the question of where synthesis should occur still remains. Despite our arguments against integrating distinct and diverse research traditions within science, there is a context where it is appropriate and commonplace to pursue the practical synthesis and integration in Stage III of Low and Altman's trajectory. Practioners, planners, and managers have to make decisions to implement specific courses of action. As a consequence of the need to take action in specific situations, the sort of pluralism that can exist unproblematically in scientific realms is not an option in the world of action.

It is not uncommon for place researchers to suggest that decisions involving relevance, superiority, or synthesis of divergent paradigmatic approaches with respect to the realm of application and practice should be made by researchers. For example, Stedman (2003) argues that a phenomenological approach may present a barrier to forest managers because "traditional forest management ... has relied more heavily on conventional positivistic (psychometric) science and its hypothesis testing approach" (p. 824). Ironically, Franck (1987) suggests just the opposite: "[phenomenology] presents more of an opportunity for [finding] a common meeting ground for architects and social scientists than positivism [psychometric tradition] can because of (phenomenology's) close attention to the essence of human experience" (pp. 66–67). Speaking from the perspective of administrative practice, however, Hummel (1991, 1994) argues that even "after the best scientific studies" administrators are faced with the question of integration and relevance and that in such judgments "science itself is not helpful" (p. 314). As he says:

> [S]ome analytic scientists confuse two operations: the analytic operation of taking reality apart and the synthesizing operation of putting reality together ... The manager's world seems to be founded on synthesis not analysis (Hummel, 1991, p. 33).

In other words, he views practice (planning, management, and design) as a synthetic act that must sift, weigh, and incorporate the findings from divergent approaches to science and other forms of knowledge. We agree with Hummel's perspective as well as with his view that "how to integrate the kind of knowledge that science can give with the practical judgment about what the [managerial] situation requires" remains one of the "great unresolved questions" (Hummel, 1994, p. 314).

Because attempts to resolve this question require an exploration of the realm of practice, a thorough treatment of this issue is beyond the scope of this paper, which has focused on the realm of science. However, this issue does signal the need for a broadening of focus and discourse that includes the realm of practice as concepts progress from Stage II to Stage III. An important distinction here is one between the evolution of scientists and the evolution of concepts within social science. Some researchers may remain committed to a particular paradigmatic tradition, maintaining primarily an internal focus their entire career, and seeking chiefly to contribute to paradigmatic advancement. However, Stage III moves into the realm of practice and creates a need for researchers who adopt more of an external, multiple paradigm focus and dialog. At a minimum, Stage III requires that researchers translate their work with sufficient clarity and transparency so that practitioners can meaningfully engage in their own synthetic endeavors, just as Halling (2002) seeks to do with respect to phenomenology. In Stage III, researchers can facilitate the demands of practice by becoming more informed themselves about alternative research traditions and by presenting their research in a way that enhances *practitioners*' ability to achieve synthesis and integration.

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Snapshots of what, exactly? A comment on methodological experimentation and conceptual foundations in place research

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Abstract

Place ideas in natural resource management have grown in recent years. But with that growth has come greater complexity and diversity in thinking and mounting confusion about the ontological and epistemological assumptions underlying any specific investigation. Beckley et al. (2007) contribute to place research by proposing a new methodological approach to analyzing attachments to place and exploring the relative importance of biophysical versus socio-cultural attributes in determining place attachment. While our thinking has benefited from their contributions to place research, we see an increasing need to clarify the multiple and competing paths for place research easily obscured in the heap of similar sounding place concepts. Our commentary cautions against philosophically unguided methodological experimentation and offers some critique of their conceptual approach to place.

Keywords: epistemology, critical pluralism, mixed methods, place, commodity metaphor.

Beckley et al. (2007) describe their paper as a "methods experiment." At the heart of their experiment is an attempt to wed an ethnographic/qualitative method of data collection with a positivistic/quantitative method of data analysis. However, we maintain that Beckley et al. are mixing, not methods, but distinct research paradigms. Their attempt to do so raises two important epistemological questions: (1) can two disparate paradigms be integrated in a single research

design and (2) what evaluative standards should be applied in the case of such a mixed paradigm research design?

Research methods are the most basic building block of research design. However, the same method can be deployed in vastly different ways depending on the research goals, researchers' judgments about the relative importance of competing threats to validity, and other epistemological assumptions. In contrast, paradigms are approaches to science that define the normative philosophical commitments that guide the actual implementation of methods. While any research design inherently requires mixing methods, attempts to mix divergent paradigms across data collection and data analysis can have significant consequences with respect to internal consistency among different methodological phases. Below, we address three inconsistencies we believe are apparent in Beckley et al.'s methodological experiment.

Beckley et al. state they chose resident employed photography accompanied by interviews as the means of data collection because this ethnographic approach provides "an extraordinarily rich device for entering into deep, detailed and meaningful conversations" (p.) about place attachment. However, this richness is deliberately sacrificed in the data analysis phase because coding adheres to positivistic criteria that prioritize simplicity. Specifically, simplicity was attained in part by the use of fairly generic codes and by coding only the primary response "[thus] simplifying the complex and inter-related meanings that were articulated" (p.). The result is that the analysis actually presented is not of the rich ethnographic database but of the highly simplified category codes. Further, the attempt to transform the rich ethnographic database into a simplified coding scheme created what appears to be a high degree of inter-rater disagreement in coding (one-third of the time at least 50% of the coders disagreed and another 30% of the time at least one of the coders disagreed).

The issue of inter-coder disagreement is related to a second apparent internal inconsistency. Ethnographic approaches use interviews to allow for thorough answers and to avoid imposing the researchers' categories on the subject. Indeed, Beckley et al. state they used interviews "to avoid constraining subjects' answers to ... predetermined responses ... taken from the literature" (p.). However, there is a significant question about whose categories (respondents' or researchers') the data ultimately analyzed actually reflect. As Beckley et al. state, "each time a category was cited by one of the four researchers, it became a data point ... data coding ... yielded four distinct sets of data, one for each researcher...." (p.). Thus, while the original interviews reflect rich descriptions in the respondents' own words, the analyzed dataset is based on what appear to be *a priori* researcher defined categories and the analysis represents the collective amalgamation of how that coding scheme should be deployed by four different researchers who were often not in agreement. Collectively this approach to data analysis seems to negate the originally stated goals of ethnographic data collection.

A final internal inconsistency stems from the explicit instructions to interviewees versus the strategy apparently followed by coders. Interviewees were instructed "to be creative" for example, "taking a picture of a gravesite to represent history". Interviews were then used so respondents could describe how the photograph reflected their attachments. Thus these instructions encouraged respondents to take photographs whose content was symbolic rather than literal and suggests the photographs and the interviews constituted a single integrated data set. In contrast, the coding scheme seems to have created a tension between the photographs and interviews that seemingly belies these instructions. For example, consider the respondent who explains a photograph by stating "the history really defines that spot for me" because it is where he "lost [his] virginity". According to Beckley et al., the analysis scheme created a dilemma as to

whether to code the photograph as biophysical (and within this category whether to code it as landscape or water) or social despite acknowledging that "the photograph was taken to depict an historical event" (p.). Thus, the coding scheme seems to have not only imposed predetermined, researcher based categories, but also to have encouraged coders to take the content of photographs literally and to treat photographs and interviews as separate sources of information with no clear priorities for which to privilege.

These apparent internal inconsistencies give rise to the second epistemological question: what standards should be used to evaluate the research? Typically, evaluative standards are developed, debated, and accepted at the paradigmatic level. To promote both diversity in perspective and fairness in the review process, a critical pluralism advocates evaluating a specific study according to the appropriate paradigmatic standards rather than the standards of some alternative paradigm (Patterson and Williams 2005). However, when paradigms as disparate as the two reflected in this study are experimentally mixed within a single research design, the question of appropriateness and fairness in evaluative standards becomes problematic.

First, positivism (which the authors indicate is the basis for the actual analysis) is often associated with an emphasis on standardization (to attain objectivity through the removal of personal judgment) and replicability (made possible in part by standardized and reproducible methods). Because disagreement among the four raters in the study seems rather high, this analysis does not seem to have a high degree of replicability through standardization. Further, positivist research values statistical inference, yet the fact that the dataset for analysis represents an aggregate of ratings of the same stimuli by four different researchers seems to prohibit the use of inferential statistics due to the problem of independence. Under these circumstances it seems likely that researchers adopting positivist standards would prefer a more traditional survey

approach in which the respondent, not a bank of researchers, interprets their experiences in light of the researchers' predetermined categories and for which widely accepted "critical tests" (e.g., reliability, confirmatory factor analysis, etc.) have been developed.

In contrast, "ethnographic" paradigms explicitly acknowledge the problematic nature of replication and inter-rater reliability, hold philosophical stances in which multiple interpretations legitimately co-exist, and therefore offer alternative evaluation standards. For example, Giorgi (1975, p. 96) suggested that a principal evaluation criterion is "whether a reader, adopting the same view point as ... the researcher, can also see what the researcher saw, whether or not he agrees with it." This standard seems more suited to Beckley et al. because the authors were often did not agree on classification of photographs but apparently could see each others' rationale since they included all coders' ratings in their analysis. The difficulty in applying this standard to Beckley et al. is that readers do not have sufficient access to these widely contested judgments to assess for themselves how warranted the coding is because the data reported are frequencies of codes rather than the data justifying actual assignment of codes.

Further, ethnographic approaches tend not to make as strong a distinction between data collection and data analysis as reflected in this study. For example, in an ethnographic interview, the role of the researcher is to probe and clear up ambiguity (e.g., is the picture about the landscape, the water, or the lost virginity) during the course of the interview rather than to withhold judgment until subsequent coding by independent raters. Alternatively, other ethnographic approaches employ member checks (reviews by respondents' interviewed) to assess the validity of coding. A further complication with regard to ethnographic standards is the aggregate nature of the analysis which transforms the rich interview data set into generic and abstract categories. Ethnographic approaches that use open-ended interviews typically emphasize

textual analyses that seek to maintain the complexity, interconnections, and richness of meanings that motivated the approach to data collection in the first place.

Thus, from an epistemological standpoint, Beckley et al.'s methodological experiment raises two problematic issues. The first stems from the mixed methods within a single research design. In actuality what appear to be mixed are different paradigmatic commitments between the data collection phase and the data analysis phase. Speaking to the possibility of integrating disparate paradigms, Franck (1987, p. 60) argued that "what one would achieve from some integration would be a strategy still based primarily in one perspective or the other'' rather than a true integration. We believe this is the situation reflected in Beckley et al. The data set that is actually analyzed seems to have lost its ethnographic character, reflecting instead a dataset primarily in accord with the tenets of positivism. And while the authors are admirably transparent about their methodological decisions, neither typical positivist nor typical ethnographic standards for evaluation appear to apply and no alternatives are presented.

Methods are not passive instruments. They (and the specific logic with which they are implemented) impose a structure on empirical systems that have significant implications for the nature of the empirical test (Danziger 1985). For these reasons, we believe that methodological experimentation should be attentive to the structure that various methodological choices impose on the data and that researchers should strive to emphasize the internal consistency across approaches to data collection and analysis within a specific research design. In contrast, in recognition the fact that any single research design is an inherently limited and imperfect representation of reality, we encourage diversity in approach across different research designs within a domain of research like place (i.e., multi-methods rather than mixed methods). To be both fair and critical, this type of pluralism requires researchers to express an acceptable set of

evaluative criteria for judging the merits of research. It is possible to do this without using established epistemological paradigms; the keys are presenting a transparent, internally consistent, and defensible testing logic that makes knowledge claims well warranted and discussing the underlying assumptions or implications of methodological choices. But adopting an established paradigm is advantageous because of the amount of prior experimental work that has gone into articulating and justifying an underlying logic, exploring internal consistency, and establishing consensus regarding standards/criteria for evaluation.

Beyond these epistemological issues, a central concern with the paper as an exemplar of place research is that, in our view, conceptually it has more in common with traditional multi-attribute economic/utility theories of human-environment relations that gave rise to place research as a critique and alternative in the first place. In making our argument it is helpful to begin by describing some of the features of place research that distinguish it from other topics.

Recent comprehensive literature reviews suggest place embodies material form, location, and something described by various terms including sense of place and place meaning (Cresswell 2004; Gieryn 2000). Material form refers to the material setting through which people conduct their lives including not just biophysical (natural properties) but also the "constructed" forms typically found in human impacted spaces (buildings, streets, offices, trails, campsites). Second, places have location and dimensionality and can be conceived as nested in scales from the subatomic to the galactic. Still, while place implies boundedness, actual boundaries are fluid and contested social constructions imposed on the world. Third, and in part a consequence of the constructed nature of spatial boundaries, what most differentiates place from other spatial-material concepts (e.g., environment, resources) is the way that place organizes and even constitutes human/social relations and meanings. Places are not only "materially carved out

[they] are also interpreted, narrated, felt, understood and imagined ... [Place meanings are] flexible in the hands of different people or cultures, malleable over time, and inevitably contested" (Gieryn 2000, p. 467). Place is more than a setting or container: it is integral to how we organize and experience the larger world, a "fundamental means through which we make sense of the world and though which we act" (Sack 1992, p. 1).

While many trace this contemporary place concept back to the emergence of humanistic geography in the early 1970s (e.g., Tuan, 1974), it is worth noting how place ideas took root even earlier in urban sociology. Firey (1945) was among the first to ascribe "sentiment" and "symbolism" to urban spaces as a direct challenge to the prevailing instrumental view in which "the only possible relationship that locational activities may bear to space is an economic one" (p. 140). Referring to the Boston Commons as a sacred object, Firey noted that "its sacredness derives, not from any intrinsic spatial attributes, but rather from its representation in peoples' minds as a symbol for collective sentiments" (Firey 1945, p. 144).

Contemporary theory and Firey's early work suggest two problems regarding Beckley et al.'s conceptual approach to place attachment and sense of place. First, they draw attention to the artificiality of distinguishing between natural/biophysical attributes and socio-cultural attributes. In particular there is nothing compelling, universal, or "robust" about limiting the biophysical category to natural features of place. Though places indeed have material form, compared to the sentiments and symbolism associated with intensely urban places such as Boston where Firey did his original work, Beckley et al. imply a rural / natural bias in how they conceive the material character of places. Recognizing that sense of place refers typically to the existence of non-material (meaning and social relations) aspects of place, it might be plausible to distinguish between the material setting on the one hand and social relations and meanings on the other.

However, this misses the point being made by Firey that it is not a place's intrinsic attributes (biophysical, social or otherwise) that make it special and meaningful, but that over time it has become a symbol for a particular constellation of meanings and relationships.

A second problem that Firey helps us to see is that the whole point of theorizing about sentiment and symbolism is to transcend the narrow calculus of an instrumental or goal satisfaction model of human behavior. Our own disenchantment with this "commodity metaphor" motivated us 15 years ago to apply sentiment and symbolism to understanding human relationships to wildlands, protected areas, and other natural landscapes (Williams and Patterson 1996; Williams et al. 1992). As the guiding paradigm in natural resource management going back to the early days of utilitarianism, the dominant way to explain environmental preferences had been to treat them as some form of a multi-attribute utility problem (e.g., in microeconomics and consumer behavior). However, in making the assertion that attachment is "produced through personal experience with socio-cultural and biophysical attributes of the spatial setting," Beckley et al. (p.) ignore the social processes embedded in culture, discourse, and other social practices of place making (Stokowski 2002) and return us to the instrumental search for "robust" if not universal environmental determinants of affect. Their approach renders place attachment and sense of place narrowly as psychological experiences *caused* by contact with specific material qualities of the place (in this case categorized as biophysical versus social attributes). Classifying photo-narratives into one or both of these broad pre-given categories (with limited reliability) offers paltry evidence to support their deterministic assertions and, as we argued earlier, appears to have more to do with what is in the photograph than the meaning behind it.

Throughout Beckley et al. there is a tendency to conflate the notion of attributes (or classes of attributes) with meanings. We take the two to be distinctly different. Attributes imply

properties, features etc. belonging to, or produced by the material nature of the place. People may disagree on whether any given place possesses some attribute (e.g., beauty), but there is an embedded assumption that there are material indicators of such attributes. In contrast we take meaning to be understood more broadly as something socially constructed and generally symbolic (Williams and Patterson 1996). Meaning is not a property of the person or the object, but a relationship between the two mediated through culture and individual past experience.

To return to Beckley et al. with this distinction in mind, meanings are literally *in* the photo-narrative co-constituted by the respondent and the investigator and go well beyond the material attributes of the place. Take the earlier example of the photo-narrative about lost virginity. To their credit, they admit difficulty coding this narrative in terms of biophysical versus social-cultural attributes. Losing one's virginity is not a biophysical or social attribute of this place (a beach), but a personal, intimate event that happened there, which has meaning to the respondent. Anyone looking at the photograph can tell it is a beach, but the meaning behind the location represented in the photograph is *in* the story, constructed from extant cultural and local (community) meanings interwoven with personal past events and transactions with that spot (it is yet another question as to what the story behind this locale says about the respondent's attachment to and/or sense of the community). The beach did not *produce* the meaning of this spot, though, for reasons possibly embedded in the narrative, it may well symbolize something significant about this person's relationship to and feelings for the community.

In the end we agree with Beckley et al. that resident employed photography provides a potentially useful tool for studying the nuances and complexities of place relationships, meanings, and attachments. But their strained imposition of quantitative structure on qualitative data and insistence on finding universal environmental sources of place attachment (or sense of

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Introduction

On May 2, 2006 during a hearing about President Bush's proposed doubling of the National Science Foundation's (NSF) budget over a 10 year period, Senator Hutchison (R-TX) initially indicated she did not think social science should be included, stating "I want NSF to be our premiere agency for basic research in the sciences, mathematics, and engineering. And when we are looking at scarce resources, I think NSF should stay focused on the hard sciences" (Mervis, 2006, p. 829). Earlier, in a September 30, 2005 presentation, Senator Hutchison, who chairs the Senate subcommittee that oversees NSF and is on the Senate Appropriations committee, suggested that social science "is not where we should be directing [NSF] resources at this time" (Mervis, 2006, p. 829). In May she proposed an amendment requiring NSF to give priority to funding programs in physical sciences, engineering, and mathematics as part of the discussions about the American Innovation and Competitiveness Act (S 2802). The issue was resolved through compromise language negotiated with Senator Lautenberg (D-NJ). Hutchison revised her amendment to indicate that NSF would not be restricted from funding research in any areas consistent with its core mission (Lane, 2006). Although this issue ultimately was resolved in a manner favorable to continued support for public funding of social science, it is yet another indicator of an emerging trend regarding the changing relationship between science, society, and governance in the US.

In natural resource management the relationship between science and governance typically is conceived in terms of the role of science in generating democratic policy. As this phrasing is intended to convey, most commonly the relationship is seen as a unidirectional flow of influence from science to policy. When influence appears to flow in the opposite direction as in the example above, concerns about inappropriate politicization of science are quickly raised. However, recent years have seen a relatively abundant flow from policy to science in a directive sense; that is, in terms of legislative and administrative mandates for science. Prominent examples include: the 1999 Shelby amendment (a rider to the fiscal year Appropriations Act requiring federal agencies sponsoring research to ensure data produced under a grant be made available to the public through the Freedom of Information Act); the 2000 Federal Policy on Research Misconduct (drafted by the Office of Science and Technology Policy, Executive Office of the President (OSTP); the 2001 Data Quality Act (a rider to General Government Appropriations Act, requiring the Office of Management and Budget (OMB) to issue guidelines to agencies for developing policy to ensure the quality, objectivity, utility and integrity of information distributed to the public); and the 2004 OMB Bulletin for Peer Review (developed by OMB in conjunction with OSTP).

With respect to natural resource management, these mandates are frequently interpreted, and all too often dismissed, as politicized acts rather than legitimate mandates. For example, while advocates describe the Data Quality Act as a means of increasing accountability in science, skeptics argue it serves the agenda of special interests seeking to impose barriers to the generation and use of science in federal agency decision making thereby diluting or effectively eliminating environmental regulatory protections (Conrad, 2003; Echeverria & Kaplan, 2003). However, these policy initiatives delve into questions at the very heart of science - how science

is practiced, what distinguishes science from nonscience or anti-science, what is peer review, what is data, how is anonymity ensured and who decides, and other issues that have significant implications for practice of science. These trends led Stankey and McCool (2004) recently to encourage natural resource social scientists to support the modification of these types of legislative initiatives in ways that recognize their legitimate purposes but reduce or eliminate unreasonable structural barriers. Similarly, in a policy review piece last year Selin and Pierskalla (2005, p. 934) encourage natural resource social scientitsts to become more proactive in strengthening "the social science voice in environmental governance."

We agree that greater professional attention to these issues is warranted in natural resource management among biological as well as social scientists. However, a key question is how to prepare to participate effectively in this arena. In the United States, there is a long historical relationship between the political concepts of democracy and science. Recent legislative mandates should be seen as part of that evolving relationship. We believe that successfully navigating these changes, requires an understanding of three separate but converging dialogs shaping the relationships among science, society, and governance. The remainder of this paper discusses these three dialogs then concludes by discussing implications, points of convergence, and future challenges to natural resource scientists.

Dialog #1: The Rise of the Administrative State, Judicial Overview, and Science

The first dialog is the one most familiar in U.S. natural resource management and centers around the emergence of the "administrative state" and growth of judicial overview originating from the Progressive political philosophy during the late 19th and early 20th centuries. It is a political discourse about the nature of democracy, the role of science in decision making, and the balance of power among the various branches of government that provides the historical foundation for understanding some of the key dimensions of contemporary debates reflected in the recent legislative mandates on policy for science.

In the US, the "administrative state" began to expand rapidly after the Civil War (Rosenbloom, 1994). The Progressivist movement became one of the predominant political philosophies shaping the development of federal administrative bureaucracies at the turn of the century, an era characterized by industrialization, rapid development of technology, and the emergence of large corporations that seemed to threaten the welfare of individuals (Rosenbloom, 1994; Williams & Matheny, 1995). It was in this social and political environment that the contemporary foundation of many federal and state natural resource management agencies were formed.

Faced with an era of rapid industrialization and, compared to today, relatively low education levels within the populace, Progressivists were skeptical about participatory decision making processes due to concerns that the complexity of issues and the advantages held by industrial special interests might subvert the ability of participatory processes to serve the public interest (Williams & Matheny, 1995). One of the Progressivists' major tenets was establishing a clear separation between politics and administration (Rosenbloom, 1994). They believed that public policy decisions were best made by competent, neutral professional administrators. However, this raised the question of how to ensure "neutral competence" in discretionary decision making (Morgan, Shinn, & Green, 2001). The Progressivist solution was to conceive of public administration as a science (Rosenbloom, 1994). In their view, discretionary policy and decision making was best put into the hands of scientifically trained experts who, alone, have sufficient expertise and familiarity with the issues and technology to understand the problems

and who, through their career professional status, "... would transcend the petty political squabbles of self-interested groups and serve the general public interest" (Williams & Matheny, 1995, p. 13).

This solution may at first seem to conflict with democratic ideals such as public participation in governmental decision processes. However, Progressivists defined democracy, not in terms of access to political participation and power, but as a societal end. For them, a democratic society was one that was characterized by equality (defined as access to at least a minimum level of material comfort and prosperity) and liberty (the ability to pursue and enjoy this material equality) (Taylor, 1992). In other words, Progressivists viewed democracy as a means of serving the public interest and argued that participatory procedures should be abandoned if public interest is better served by alternative decision making processes (Williams & Matheny, 1995). For example, Gifford Pinchot, who spearheaded the development of the Forest Service during this period, was a strong adherent of Progressivist philosophy. As a Progressivist, he viewed publicly held natural resources as material resources to be used to help achieve a just and democratic political order (liberty and equality) (Taylor, 1992).

However, the rise of the administrative state and its development under the Progressivist philosophy also were seen as challenges to the separation and balance of powers underlying the design of US government (Rosenbloom, 1983). During the New Deal era, these tensions led to political confrontations among various branches of government. The political confrontations were tied more directly to spheres of economic and social policy other than natural resource and federal land management during this era. However, the outcome of these challenges had implications for the evolution and nature of the administrative state in relation to natural resource management which became more evident in the 1960s because they ultimately shaped the legal contours of Congressional delegation of authority to the executive branch, the nature of judicial review, and the rights of the public with respect to agency decision making.

During the New Deal one of the primary tasks of those supporting the continued expansion of the administrative state was to establish a more centralized government in which the executive branch attained supremacy over Congress and the Court (Rohr, 1986). This shift represented a challenge to the traditional role and standing of judicial authority in the realm of society and economy and did not go without a judicial response, reflected in battles between the President and the Supreme Court over New Deal policies (Rohr, 1986; Rosenbloom, 1994; Witt, 1990). Congress too sought to negotiate its evolving role with regard to the development of the administrative state during this period. Realizing that expansion of the executive role in economy and society would require increased delegation of legislative authority, Congress sought to ensure that this newly delegated authority would not be abused, but exercised in a manner consistent with Congressional intent (Rosenbloom, 1997). Along this line, Congressional critics of the New Deal administration successfully passed the Walter-Logan Bill in 1940 (Rohr, 1986; Rosenbloom, 1997). According to Rohr (1986, p. 157, 165), this bill instituted rigid judicial control that essentially would have made "administrative agencies wards of the courts." Roosevelt vetoed the bill, justifying the veto in part based on an upcoming Attorney General's report exploring the question of regulating administrative agencies. The report was issued in 1941. World War II delayed the debate over its implementation but ultimately, in 1946, Congress passed the Administrative Procedures Act (APA) which was informed by the attorney General's report (Rosenbloom, 1997).

Under the APA, all federal agency actions are subject to judicial review except where review is specifically precluded by statute or where an action is "is committed to agency

discretion by law" (§701(a)). (Subsequent Supreme Court rulings limit the latter exceptions to those rare cases where statutes are so broadly written that there is no law to apply (Lindstrom, 2000).) Section 706 of the APA defines the judicial scope of review, instructing courts to hold agency action unlawful if it is:

> "(A) arbitrary, capricious, an abuse of discretion, or otherwise not in accordance with law; (B) contrary to constitutional right, power, privilege, or immunity; (C) in excess of statutory jurisdiction, authority, or limitations, or short of statutory right; (D) without observance of procedure required by law; (E) unsupported by substantial evidence in a case [in the case of formal rules or hearings] ...; or (F) unwarranted by the facts to the extent that the facts are subject to trial *de novo* by the reviewing court."

Overall, the act also seeks to ensure that agency rulemaking resulting from Congressional delegation of authority is at least marginally participatory and has been referred to as a "bill of rights" for an administrative state because the ground rules for judicial review it establishes seek to provide a balance between administrative discretion and protection of individual rights in administrative actions (Rosenbloom, 1994). It seeks a balance between the strict form of judicial review in the Walter-Logan Bill which many perceived would make the agencies simply a conduit for court decision making versus a court that merely "rubber stamps" agency decisions leaving the public "to the whim of the administrative state" (Nelson, 2003, p. 181; Rosenbloom, 1997).

Up to this point, the struggles among the various branches of government regarding roles in the administrative state may seem to have limited implications for the conduct of science in environmental governance. However, the framework established by the APA in conjunction with changes in the nature of society which became politically evident in the 1960s brought science in natural resource governance back into sharp contemporary focus. Especially with respect to natural resource management, the 1960s were characterized by a strong challenge to the Progressive era concept of democracy and decision making. The public of this era showed an increasing distrust in governmental agencies, demanded a greater role in natural resource decision-making, and expressed a desire to see a broader range of societal values addressed in natural resource planning and policy (Dana & Fairfax, 1980; Shannon, 1981). These concerns led to significant legislatively mandated changes including passage of the National Environmental Policy Act (NEPA) in 1969. The fusion of this social unrest, the APA, and NEPA, gave rise to a social and political environment in which science has made a frequent appearance in legal suits challenging agency decision making.

On the surface, legal challenges to natural resource agencies' use of science would seem to have little chance of success and therefore few implications for agency science. Theoretically, at least, the standard of review with regard to the use of science by agencies does grant a great deal of discretion to the agency. For example, Earth Island Institute v. United States Forest Service (351 F.3d 1291, 9th Circuit; 2003) states "because analysis of scientific data requires a high level of technical expertise, courts must defer to the informed discretion of the responsible federal agencies"; Marsh v. Oregon Natural Resource Council, Inc. (490 U.S. 360, 377; 1989) noted "[w]hen specialists express conflicting views, an agency must have discretion to rely on the reasonable opinions of its own experts, even if a court may find contrary views more persuasive."; and Laguna Greenbelt, Inc. v. United States Department of Transportation (42 F.3d 517, 526, 9th Circuit; 1994) stated "NEPA does not require us to decide whether an EIS is

based on the best scientific methodology available". And a recent analysis indicated that from 1989-2000 the Forest Service did win 73% of its cases (Keele, Malmsheimer, Floyd, & Perez, 2006).

However, a simple characterization of the court's standards for science is not possible. For example, despite the fact that courts supposedly follow precedents and theoretically defer to agency expertise in matters of scientific method, it is still possible for different courts to find the same methodology valid in one instance but not another (see Inland Empire v. United States Forest Service, 88 F.3d at 761, 9th Circuit; 1996 which found using habitat as a proxy for monitoring population trends a valid methodology versus Idaho Sporting Congress v. Rittenhouse, 305 F.3d 957, 972, 9th Circuit; 2002 which ruled that it was not). Or, as another example, it is possible for a court to require preparation of a new Environmental Assessment (EA) in part because of the possibility of bias due to the circumstances in which the original EA was produced, while not actually identifying anything biased or wrong in the original EA (see Metcalf v. Daley, 214 F.3d 1135, 9th Circuit; 2000). As a result of examples such as these, critics have described judicial review of administrative law under APA and NEPA as schizophrenic (Nelson, 2003), a "disorderly mess of ambiguous and overlapping standards" (Young, 1996, p. 181) that has "lent itself to sloppy handling by the Court ... thereby engendering imprecision and confusion" (Anthony, 1996, p. 26). In contrast, Rosenbloom sees "a certain coherence in judicial response to the rise of the administrative state" (1997, p. 103) which has created a partnership between judges and administrative bureaucrats in which administrative law functions as a means of balancing public administration with the contractarian values of constitutional democracy (Rosenbloom, 2003).

Whichever perspective one holds on the state of administrative law under APA and NEPA, clearly, the framework of agency accountability to judicial review established by these statutes (presence of arbitrary and capriciousness, compliance with mandated procedures, transparency in agency decision making, ascertaining whether decisions are warranted by the facts) creates the possibility for public stakeholders to challenge the practice of science within agencies. And despite the existence of standards of review that imply courts should show deference to agencies in areas of science, these standards provide a great deal of flexibility about the amount of discretion that can be allowed and the issue is constantly negotiated in court cases and among legal commentators. In fact, some commentators (King, 1995; Madden, 2003) go so far as to argue that court defined standards for what counts as science in an "admissible evidence" sense (e.g., testimony about DNA evidence, epidemiology, etc. see Daubert v. Merrill Dow Pharmaceuticals, Inc. 509 U.S. 579; 1993) should be applied to science in administrative decision making despite skepticism among other commentators about the adequacy of Daubert as a characterization and means of evaluating science (Jasanoff, 1995; Meyer, 1999). Whatever one's position on these issues, it is clear that debates about democratic governance, administrative decision making, and science have been ongoing from the emergence of federal natural resource management agencies. Recent legislative mandates regarding policy for science such as the Data Quality Act, the Shelby amendment and debates about sound science in relation to the Endangered Species Act are, in part, among the most recent events in this stream of discourse.

Dialog #2: Science as a Public Good and the Social Contract for Science

The preceding historical stream is only part of the background necessary to understand the political and social factors underlying recent legislative mandates regarding policy for

science in the U.S. A more complete understanding requires examination of a second historical stream that also has shaped the relationship between science and democratic governance. The origins of this second stream are somewhat more distant from natural resource management than the preceding one, but it has significantly influenced professional and public norms regarding the appropriate relationship between science and politics in the U.S. And this stream has moved increasingly closer to natural resource management through recent legislative actions. A useful starting point for considering this second historical stream is the publication of Vannevar Bush's (Director of the Office of Scientific Research and Development) *Science: The Endless Frontier* in 1945. Along with John Steelman's report, *Science and Public Policy*, Bush's report implicitly established what is commonly referred to in science and technology studies as the "social contract for science" for the normative guidelines it sets between the elected government, which publicly funds research, and university and public agency scientists to whom the conduct of the research is delegated (Boland, 2002; Chopyak & Levesque, 2002; Guston, 2000; Guston & Keniston, 1994).

This implicit contract was established primarily with an eye toward basic science under the view that basic research represented a public good (Chopyak & Levesque, 2002; Fuller, 2000; Guston & Keniston, 1994). This perspective emerged primarily in regard to the realms of defense, health, and economics (House_Committee_on_Science, 1998). This orientation, along with the focus on "basic science," helps explain its greater distance from natural resource management. Still, the balance the social contract sought to achieve between freedom of inquiry versus political accountability have carried over to other realms like natural resource management, especially with the respect to its normative influence in shaping scientists' views about the appropriate relationship between politics and science.

Under the post-World War II social contract for science, basic science was justified as a public good, on the basis of what has been referred to as an "assembly line model" of science and technology (Hiskes & Hiskes, 1986). According to this perspective, basic science ultimately leads down a path to development of technological products useful to society or to policy-relevant research (Chopyak & Levesque, 2002; Guston, 2000; Hiskes & Hiskes, 1986). However, the relationship between basic science and productivity was more assumed rather than tracked directly as a means of evaluating accountability. Instead, the focus for discussions regarding accountability centered on the nature of basic science itself. Specifically it sought to deal with what is sometimes described as the "principal/agent dilemma," a situation in which the agent (scientists) know more about performing a task than the principal (government/the public) who hires the agent (Guston, 2000). Because of the specialized knowledge needed to conduct basic research, scientists were viewed as knowing more about both performance and the appropriate goals of research (Boland, 2002; Guston, 2000). Under these circumstances there are two potential problems: adverse selection (the principal lacks sufficient knowledge to choose the best agent) and moral hazard (the principal lacks sufficient knowledge to guard against an agent's misbehavior in fulfilling its task) (Guston, 2000). The resolution to this dilemma stemming from Bush's *Endless Frontier* was to rely on the community of scientists themselves. The role of funding research would lie with the government, but accountability would lie with the scientist (Boland, 2002). Adverse selection pitfalls would be avoided by delegating choice of agents (scientists, research proposals, research agendas, publication) to the community of scientists themselves through internal peer review (Boland, 2002; Fuller, 2000; Guston, 2000; Guston & Keniston, 1994). This self-regulation in conjunction with the inherent objectivity of science were thought also to sufficiently address the moral hazard problem (Guston, 2000). The

result was a social contract that emphasized granting scientists "creative separateness from involvement with goals, values, and institutions other than [their] own" (Yankelovich, 2003).

Over time, however, both society and government have changed in a variety of ways relevant to this issue. One of the major changes was the end of the Cold War era which was in important element underlying assembly-line view that justified public investment in basic science under the old contract (Guston & Keniston, 1994). And as described above with respect to Progressivism and the administrative state, since the 1960s, a more well-educated public began to show an increasing desire to be more actively involved in many areas of administrative decision making rather than to concede decision making entirely to elected representatives or career civil servant experts (Boland, 2002; Shannon, 1981).

With respect to government, the years since the 1945 Bush report saw the rise of the "management presidency" (Guston & Keniston, 1994, p. 17). Most notable among the changes was the formation of OMB from the Bureau of Management and Budget in 1970. OMB's realm of influence includes not only budgetary issues, it also supervises rule and policy making within agencies as well as the development and use of public surveys and other forms of information collection (Guston & Keniston, 1994; Rosenbloom, 2003). And since 1946, Congress has reorganized itself to provide greater oversight for federal agencies, creating the Congressional Research Service and giving greater capacity to the General Accounting Office (recently renamed the General Accountability Office) for investigating federal administration among other initiatives (Rosenbloom, 2003).

As a result, though the social contract for science was always implicit (Guston, 2004) and there was never a truly "golden age" in which science was completely severed from political accountability within the government (Guston & Keniston, 1994), there is growing evidence the implicit social contract has dissolved and a new one is being negotiated (Demeritt, 2000; Guston & Keniston, 1994). Perhaps the clearest evidence for this is the 1998 policy statement by the House Committee on Science, *Unlocking Our Future: Toward a New Science Policy: A Report to Congress*. The report acknowledges Bush's *Science: The Endless Frontier* as the foundation of US science policy, but it also clearly signals a change in the implicit social contract. For example, though acknowledging the importance of basic science, it changes the assumption that basic research will lead to technological innovations and improved social policies into an explicit goal:

> "[federally funded science should help] society make good decisions ... [providing] every citizen - not only the scientists who are engaged in it – with information necessary to make informed decisions as voters ... & policymakers. ... stronger ties between this enterprise and the American people must be forged."

The report also raises the notion of external accountability, pointing to the Government Performance and Results Act (Public Law 103-62) (GPRA) as a legislative mandate "developed for the purpose of providing such accountability."

By virtue of linking its policy statement on science to Bush's 1945 policy statement, the House Committee report on science represents the most explicit statement of the renegotiation of the implicit social contract. However, we believe that the Data Quality Act, Shelby amendment, and Federal Policy on Research Misconduct also are, at least in part, symptoms of this renegotiation rather than merely attempts by special interests to subvert regulatory guidelines as some critics portray them.

Dialog #3: Debates about the Nature of Science Among Scientists

The final dialog relevant to the ability to participate meaningfully in evolving discussions about the relationships among science, society, and governance is one that primarily has been carried on between scientists (including fields that take science and scientists as their subject of study - philosophy/sociology/history of science). With respect to this issue, there is not a single integrated discussion among scientists with regard to the nature of science. Rather there are many different discussions reflecting different fields and subcultures of scientific practice. These discussions deal with standards for science, often focusing on the question of demarcating science from "nonscience" or "anti-science." Examples include the tension between "evidence based medicine" versus "traditional clinical practice" in medicine (Havighurst, 2001; Tanenbaum, 1994), debates between qualitative versus quantitative research in the social sciences (add appropriate citations), criticisms and defenses of case study approaches within the policy sciences (Box, 1992; Hummel, 1991) , and the 1990s "Science Wars" between a subgroup "realists" (mostly in the natural sciences) and a subgroup of "postmodernists" (mostly in the humanities and social sciences) (Jasanoff, 2004; Segerstrale, 2000; Soule & Lease, 1995). When centered around the demarcation question, these debates have the appearance of a Kuhnian model of scientific evolution in which standards accepted as normal science are confronted and ultimately replaced in a revolution-like process (Kuhn, 1970, 1977).

However, though differences in philosophy of science have not been thoroughly resolved (and likely never will be), when these dialogs are considered in a more collective fashion, increasingly it appears that the resolution will not be a revolution in the Kuhnian sense, but the emergence of a more pluralist perspective on science (Bocking, 2004; Dupre, 2002; Haack, 2003; Patterson & Williams, 1998; Pedynowski, 2003). This perspective focuses more on the fit of a given research logic to the subject of investigation than on the distinguishing factors of science. For example, Larry Laudan (1996) discusses "the demise of the demarcation problem" due to factors such as: recognition of the "fallibilistic" character of science (p. 213); the historical failures of attempts to define logically defensible normative demarcation standards, the recognition that what is accepted as science "in our culture ... is not all cut from the same epistemic cloth" (p. 221) and therefore science entails conceptual as well as empirical problem solving, and evidence that various theories are accepted "according to quite diverse standards" rather than on the basis of "universally shared standards" (p. 235). He argues that instead of focusing on the question of what makes a belief scientific, the philosophically interesting and tractable problem is rather what makes a belief well-founded in terms of underlying empirical and conceptual credentials? Haack (2003, p. 24) similarly notes that science is fallible and imperfect, "that there is no mode of inference, no 'scientific method', exclusive to the sciences and guaranteed to produce true, probably true, more nearly true, or more empirically adequate results." Like Laudan, Haack (2001) argues the focus should be on how well warranted a claim is. Warrant, in her view, depends on how well a claim is supported by empirical evidence and background beliefs, the reasonableness of the background beliefs, and how well the claim is explanatorily integrated with the evidence.

While the preceding two examples come from scholars within the philosophy of science, they are not inconsistent with ideas found among individuals more closely linked to the actual practice of science in a diverse array of fields. For example, in a recent review article on the use of structural equation modeling in psychology, MacCallum and Austin (2000, p. 218) state:

> "Our review produced a variety of concerns about assessment of model fit and interpretation of parameter estimates... researchers do not seem adequately sensitive to

the fundamental reality that there is no true model ... that all models are wrong to some degree ... and that the best one can hope for is to identify a parsimonious, substantively meaningful model that fits observed data adequately well. At the same time, one must recognize that there may well be other models that fit the data to approximately the same degree. Given this perspective, it is clear that a finding of good fit does not imply that a model is correct or true, but only plausible."

Similarly, writing about ecology in *BioScience*, Allen et al. (2001, p. 475) argue that the view of science as something that seeks out or can find a "single quantified truth" is myth. They argue that rather than retreating from post-positivist critiques of realism, ecologists should reconceptualize evaluative standards in science focusing on concepts like quality (meticulous application of methods that are subject to continual self-reflective challenges) and narrative (a complex concept with parallels to MacCallum and Austin's suggestion there is no true model, Laudan's suggestion that not all science is cut from the same epistemic cloth, and Haack's discussion of background knowledge and integrated claims). Similarly, writing from an interpretivist based perspective in reference to analysis of qualitative data, Mishler (1990) argues that a key evaluation criterion is that readers can make a reasonable judgment about the warrants for the researcher's interpretive claims.

The shift to a more pluralistic view of science illustrated in the perspectives presented above share in common several themes which are increasingly reflected in discussions of science within the community of scientists, two of which are relevant to the current paper. First, they hold that phenomena studied by scientists differ markedly in their nature and therefore no single set of methods or epistemological rules are universally suited across the breadth of scientific endeavors. Second, they represent a shift from a foundationalist to an anti-foundationalist philosophy regarding evaluative criteria in science. A foundationalist philosophy treats validity as if it were an are objective, measurable component that can be ensured *apriori* through successful adherence to standardized methodological procedures (Holt, 1991; Patterson & Williams, 2001). In contrast, an anti-foundationalist philosophy maintains that "the credibility of the interpretation cannot be inferred separate from its reading" (Holt, 1991, p. 59). In other words, the focus is on evaluating the product: is the research logic defensible given the background assumptions?, are the interpretations warranted given the empirical evidence presented?, are the claims made appropriately bounded given the study limitations?, is the narrative coherent and persuasive?, etc.

While anti-foundationist philosophy may seem somewhat nebulous due to lack of specific criteria regarding methodological techniques or measurable outcomes, it does have direct implications for how science is practiced. Changes from foundationalist philosophies to an anti-foundationalist philosophy have implications for the definition and normative implementation of fundamental scientific concepts like objectivity. For example between the 17th and 19th century, attempts to demarcate science from nonscience were grounded in a view that science (as opposed to fallible opinion) would be built on infallible foundations of universal laws (Laudan, 1996). Under this philosophy objectivity was understood as value-free observations, what Allen et al. (2001) referred to as a Dragnet or "just the facts Ma'am" view. However, Laudan notes that this view of demarcation began to "unravel" in the 19th century as an epistemological perspective reflecting uncertainty and acknowledgement of the fallibilistic nature of science became the more dominant view. From this perspective, the demarcation principle became focused on method, promoting the view that what distinguished science from other activity was its methodology (Laudan, 1996). Under this perspective, objectivity came to

be understood more in terms of what Porter (1992) called the "accounting ideal" of objectivity. Under this perspective, the goal of objectivity was still to eliminate personal bias, however the mechanism by which this was accomplished was grounded in the notion of methodology and communication rather than in discovery of universal natural laws and truth. Specifically, efforts to ensure objectivity emphasized eliminating personal bias with explicit, standardized methodological rules that eliminate judgment (Porter, 1992). Under this view, quantification and statistics were seen as especially effective ways of ensuring public, depersonalized, objective communication to the extent that many equate science with these methodological techniques. Replication of experiemental results following standardized procedures becomes the scientific ideal. However, mathematics and statistics are not passive instruments, they impose a structure on empirical systems and not every phenomenon of interest can be represented quantitatively (Danziger, 1985; Porter, 1992). And many interesting and complex social and ecological research questions cannot be studied in tightly controlled experimental designs, in which case replication is not always a possibility.

Recognition that mathematics/statistics are not neutral and that not all research problems are "cut from the same epistemic cloth" (Laudan, 1996, p. 221) are factors that have contributed to the emergence of a more pluralistic and anti-foundationalist view of science as noted above. This in turn once again has shifted perspectives on objectivity. Mitchell (2004), for example, argues that objectivity should be understood in terms of public exposure and transparency needed to facilitate scientific dialog by providing the reader a sufficient basis for ascertaining whether "a particular observation or inference offers good evidence for an empirical proposition" (p. 182). This essentially is the anti-foundationlist point of view. Objectivity is understood as publicly communicated transparency in conceptual frameworks, underlying assumptions, research logic, specific methods, and data sufficient for readers to make relatively independent judgments of the warrants for interpretations and to facilitate meaningful dialog. This anti-foundationalist understanding of objectivity is not an uncritical or radically relativist and individualistic one. At the same time it is suited to many scientific research logics beyond statistical and experimental ones and to ecological and social situations where replication is not a possibility. And its emphasis on transparent communication helps shape understandings of the nature of peer review. From this point of view, the role of peer review is not ensuring truth. Nor is it simply one of ensuring quality. Rather, in addition to quality, it ensures sufficient transparency to facilitate public analysis and dialog which are the heart and soul of scientific progress (Giorgi, 1975; Mitchell, 2004; Patterson & Williams, 2001; Wachterhauser, 1986).

As noted above, there is not yet a final consensus or resolution among the diverse communities of scientists involved in this dialog regarding alternative views of science. However, there does appear to be an emerging trend toward pluralism and anti-foundationlism. Further, while this third dialog has been primarily among scientists and those who study science rather than within the legislative, administrative, or legal arenas, it does have implications for recent policy for science initiatives in these external realms. For example, in the Courts, one epistemological logic (falisificationism) has been identified as a defining criteria of science (most significantly in the Supreme Court ruling in Daubert v. Merrill Dow Pharmaceuticals, Inc. 509 U.S. 579; 1993 but also earlier in McLean v Arkansas Board of Education 529 F. Supp. 1255; 1982 – a case dealing with the scientific nature of creation science) despite concerns among many contemporary scholars of science about its adequacy as a universal normative characterization as science (Chalmers, 1982; Haack, 2001; Jasanoff, 1995; Laudan, 1996). Or, as another example, the Data Quality Act required OMB to develop guidelines for agencies to

ensure data/information used maximizes objectivity. This raises the question of what view of science is reflected in OMB's more specific definition of objectivity? And will this view of objectivity be maintained in agency's subsequent reinterpretation and implemention?

Conclusion

Recent legislative mandates regarding policy for science should be understood as reflecting a societal debate that runs deeper than merely than attempts by special interests to subvert science through political avenues. Institutions are created in social contexts in response to the values, goals, and conflicts in a particular culture at a given point in time. But over time, the original social contexts that give rise to institutions change as technology, values, goals, and meanings change. As a result, institutions must also evolve and adapt if they are to continue to function successfully in that culture (Patterson, Montag, & Williams, 2003). Science is not an exception, it evolves as a result of both external and internal pressures. Contemporary external pressures, stemming from the first two dialogs reviewed above in which society is seeking to place appropriate checks and balances on administrative agencies as part of our democratic system of governance (dialog #1) and to renegotiate its relationship with science as a publicly funded good (dialog #2) , are leading to a rise in policy for science initiatives. These have implications for the practice of science. Internal pressures stem from the emergence of a more pluralistic perspective on scientific epistemology reflected in the third dialog.

In the face of these pressures for change, a key question is how should natural resource professionals and scientists engage in these discourses. Selin and Pierskalla (2005; 2006) encouraged natural resource social science professionals to become more actively engaged in these issues by serving on advisory councils, organizing a "Social Science & Policy Committee" within the International Association of Society and Natural Resources that develops position statements and links with similarly oriented groups like the Consortium of Social Science Associations, and developing education/outreach programs that target resource managers and train them about social science. We agree that greater attention to these issues within the profession is crucial, and wish to further emphasize the need for internal dialog, education, and self-reflection both when and across disciplines (social and natural sciences). To participate effectively natural resource scientists must understand the nature of the societal dialogs we are seeking to influence. The Progressive Era view of science in governance has met its demise. The old social contract for federally funded basic science appears to be evolving. We need to be aware of these issues to be able to participate effectively and to avoid counter-productive scape-goating that underlay the Science Wars of the 1990s. (Segerstrale (2000) notes that some commentators have suggested that part of the antagonism that was directed internally among different communities of scientists actually was a response to external, societal sources such as decreased legislative support for funding basic science).

Particularly important is a critical self-examination of both our process for educating professional scientists and our system of institutional rewards. For example, the relationship between interdisciplinary research and interdisciplinary education has recently been debated in the Forestry Chronicle (Anderson, 2005; Innes, 2005). Anderson expresses a view that seems prevalent among many scientists – that cutting edge science "requires solutions to problems so intricate and complicated that only those on the frontier of their discipline will likely make a contribution, which is why … graduate level curricula should, for the most part, remain disciplinary" (p. 786). His perspective is influenced by both his view on the goal of science (contributing to "knowledge making", p. 785) and his sense that in the "publish or perish" world

of academic research, generalists are less successful than specialists. In many ways this view suits the reward system at most research universities – there tends to be a hierarchy that rewards publication in basic science contributions more highly than applied journals or professionally oriented journals. This, combined with the exponential growth of information and papers in any specific areaencourages specialization. However, recent legislative trends reflected in the changing social contract with science and carrying implications for the future of government funded science (e.g., the House Committee Report on Science, 1998) emphasize problem solving and relevance to society over mere knowledge making as a goal. Further, a danger of over-specialization is that it is more likely to promote a community of specialized scientific practitioners lacking in understanding of how their work relates to societal dynamics. This is a increasingly perilous situation given recent trends in legislatively and administratively mandated policies for science because these policies have implications for how science is practiced in addition to what is funded. So the professional challenge becomes how to keep communities of specialist scientists integrated with the societies that support and benefit from their work. This article sought to contribute toward this effort by outlining the different social dialogs that are converging around these issues. However, we believe the questions of how to provide sufficient understanding and involvement in these evolving dialogs within the scientific community is an area that needs greater attention within the profession.

In their recent review of the state of social science in natural resource management, Stankey and McCool (2004) also encourage the profession to look to a fundamental reform of the educational process as a means of building capacity to tackle and implement integrative approaches to problem solving. Additionally, Stankey and McCool encourage social scientists to "learn the culture, norms, and language of the biological, physical and resource management communities." We also encourage increased professional attention to this latter recommendation. Knowledge comes in a variety of forms (e.g., wisdom, scientific knowledge, information, outformation) with different characteristics (public accessibility, objectivity, situatedness/contextuality) (Ezrahi, 2004). And knowledge resides in a variety of different institutional contexts (e.g., university research, administrative/managerial decision making, legislative policy making, courts). Across these institutional contexts, the generation, management, use and standards of evaluation of knowledge, though similar in some respects, also differs. For example, Hummel (1991) differentiates the scientific from the administrative/managerial realms in that the former has often focused on objectivity, replicability and general knowledge, and analysis while the latter realm focuses on inter-subjectivity, uniqueness and context of specific cases, and synthesis. Or, as another example, in discussing the difference between knowledge standards in science and those in the court, Leiter (1997) suggests there are good epistemological reasons for keeping these standards separate due to the different goals. Science often looks for the most parsimonious and reasonable account and continually challenges ideas while the courts give relatively greater emphasis to efficiency and individual well-being in a more highly bounded timeframe. Overall, Leiter (1997) encourages "epistemic realism." By this he means that the standards and epistemic norms employed in a particular social realm should be adapted in a way that recognize the real-world epistemic character and limits of that particular realm. Thus, we believe that Stankey and McCool's (2004) suggestion for the need to understand the norms of different communities should be understood broadly to include the broad array of realms involved in the translation of knowledge to societal application (Figure 1).

We believe work in this area represents one of the next frontiers in the maturation of the social science in natural resource management. In fact, we see the goal of developing a better understanding of how knowledge migrates and is evaluated across these different realms as one point of convergence between recent policy for science initiatives and the interests of contemporary natural resource social scientists. For example, Senator Hutchison, whose comments regarding social science and NSF introduced this review, also has made comments indicating that she is not wholly against social science being part of NSF's mission. And examples of "good, solid social science research" she cited included studies of organizational processes and knowledge transfer and research to understand resistance to innovation in science teaching (Anonymous, 2006). And John Marburger (2005) (Director of the Office of Science and Technology) has called for economists and social scientists to focus on exploring the effectiveness of science policy. While it is likely that the detail-specific definitions of the underlying issues, views on how these problems should be approached, and intentions regarding how the information may be used differ across Senator Huchison, Marburger, and Stankey and McCool, this is always the case in when political and scientific realms overlap.

There appear to be a number of other points of convergence as well. For example, the 1998 House Committee on Science Report emphasizes the relevance of publicly funded science: "Science ... can provide every citizen – not only the scientists who are engaged in it – with information necessary to make informed decisions as voters, consumers and policymakers". This interest is consistent with an emerging trend in social science to pay greater attention to the goal of understanding context and cases rather than simply generating universal principles and laws. This trend is reflected in a number of recent discussions. For example, Bent Flyvbjerg's (2001) recent book on research in social science focuses on how to enhance the relevance of social research. Authors seeking to establish anti-foudationalist standards for evaluating research have emphasized practical utility as one of the central criterion (Packer & Addison, 1989; Patterson & Williams, 2001), while Halling (2002) explores how to make phenomenology more accessible clinicians, policy makers, and ordinary citizens. In fact, the final stage in Low and Altman's (1992) evolutionary trajectory in the maturation of concepts in social science includes an emphasis on the "application of knowledge to the solution of practical problems" (p. 3). And finally, one of the six major critiques of the 1998 House Report by the American Association for the Advancement of Science was that a Congressional science policy needed to be more attentive to "strengthen[ing] mechanisms for involving those whose lives are affected by the results of research in shaping [science and technology] policies" (AAAS, 1999).

Similarly, there appears to be some convergence in how the courts evaluate agency's use of science in natural resource management and the concept of objectivity as understood from a pluralistic/anti-foundationalist perspective on science. In the court system, NEPA provides the foundation under which much agency decision making is evaluated. NEPA requires that agency decision be informed and transparent, but does not mandate a particular substantive decision. Likewise, as discussed above, an anti-foundationalist understanding of objectivity emphasizes transparency in communication to allow external audiences to make a relatively independent judgments about the merits of the study rather than adherence to a particular research logic.

Increasing the capacity of natural resource scientists to see these types convergence and capitalize on them to the mutual benefit of all parties is an important professional goal in the continually evolving relationship between science and society. At the same time, maintaining epistemic reality (recognizing the different realms in which knowledge is generated, evaluated, and applied) and ensuring that standards from one realm do not inappropriately intrude on the

standards for another are equally important challenges. Developing an understanding of the historical societal dialogs that converge with respect to these issues is a first step toward increasing our professional capacity to address these challenges. However, navigating these tensions will always remain an on-going task much in need of creative professional attention.

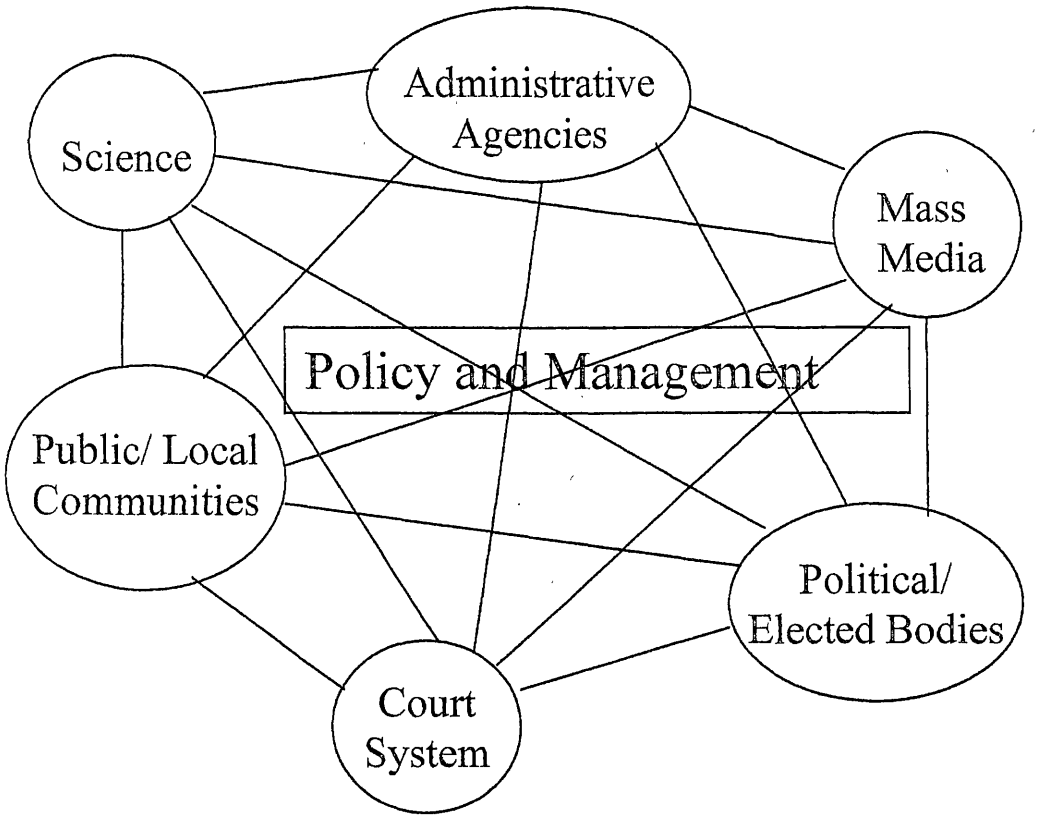

Figure 1. Broad array of realms involved in the generation, evaluation, and translation of knowledge to societal application.

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mental change and the ensuing challenges for the conservation of nature. The final section calls for a conservation biogeography that is concerned with conserving species and their evolutionary potentials through an understanding of the dynamics of distribution change and learning how best to conserve and manipulate distribution areas.

As someone trained as a biogeographer 30 years ago, I was at first disheartened to see how far I had fallen behind in my discipline. However, the more I read, the more stimulated and encouraged I became by these accessible introductions to new technologies and perspectives. I can say from first-hand experience that the enthusiasm expressed in this overview of modern biogeography and its future directions was maintained at the Second Biennial Meeting of IBS in January 2005. I recommend anyone who thinks they might be a biogeographer at heart, if not in title, to read this book. It should be required reading for every graduate student in a biogeography-related program and at least consulted by any conservation-minded person with a solid background in science.

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CORRECTION LINES: ESSAYS ON LAND, LEOPOLD, AND CONSERVATION.

By Curt Meine. Washington (DC): Island Press. $50.00 (hardcover); $25.00 (paper). xiv + 296 p; ill.; index. ISBN: 1-55963-731-5 (hc); 1-55963-732-3 (pb). 2004.

This is a collection of thoughtful and well-written essays by an important conservation biologist, environmental historian, and conservation activist. His insights and reflections about current directions in conservation science, policy, and politics are well grounded in an understanding of the historical and social contexts of American conservation, the fertile ideas of Aldo Leopold, and the integral relationship between biodiversity and the human community.

The essays are separated into the following parts: Part One consists of three essays on conservation history and development; Part Two includes five essays on Leopold's legacy; and Part Three presents three essays on the conservation path that lies ahead. Part One contains an excellent summary of conservation in America since the 1850s that shows how the development of the sciences and conservation are related; a valuable perspective on the evolution of American conservation and its relationship to the growth of environmentalism; and a useful historical account of the rise of conservation biology and the idea of sustainability. Part Two shows how Leopold sought to integrate the concepts of utility and aesthetics in his philosophy; emphasized the importance of biodiversity; stressed the aesthetic value of cranes and lent his voice to crane conservation; prepared, revised, and finally published his landmark classic, *A Sand Country Almanac and Sketches Here and There* (1949. New York: Oxford University Press); and has been received by a great variety of colleagues, critics, philosophers, environmentalists, and ordinary folk, sometimes in the most surprising ways. Part Three considers the many direct and indirect effects that the land-survey system has had on our society, both good and bad, including its consequences for biodiversity; identifies ten significant "challenges" that the land ethic will face if it is to remain vital; and concludes with a fairly stark assessment of the impact that the events of September 11, 2001 have had on our notions of security and conservation.

Biologists will find much of value in this book that will connect with their own work not only to other sciences, but also to the social and human side of conservation. Moreover, the author raises many difficult questions about what role conservation has played in our society and how it might be better conceived and implemented. In the end, he concludes that conservation is not yet a priority for most Americans, it is in fact in a crisis of caring, and that the land is narrowly defined as a raw resource at higher levels of politics. He asserts that we still face the question: "Who speaks for the American land and its people *together*?"

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NATURE'S EXPERTS: SCIENCE, POLITICS, AND THE ENVIRONMENT.

By Stephen Bocking. New Brunswick (New Jersey): Rutgers University Press. $65.00 (hardcover); $24.95 (paper). x + 298 p; index. ISBN: 0-8135-3397-X (hc); 0-8135-3398-8 (pb). 2004.

The author explores the question of how to make environmental science more effective and relevant in the realm of environmental politics. Chapter 2 discusses reasons for the declining authority of science in contemporary society. Bocking notes issues such as the rise of adversarial litigation as a means of debating science, the increased willingness of nonscientists to critique and challenge scientific experts, the appropriation of terms such as "sound science" by the private sector to serve specific interests, and greater public awareness of how funding by industry influences the character of research as contributing factors. Through Bocking's discussion, one realizes that the "science wars" are not simply an academic debate between a group of science practitioners and a particular school of

thought within science and technological studies. Rather, challenges to traditional notions of scientific authority are an aspect of political life when science confronts society through environmental policy.

Chapter 3 explores the relationship between ecological science and environmental values. The author considers arguments that ecology should be a basis for defining environmental values and concludes that this perspective is too problematic. Rather, he argues that environmental values should be defined through a more democratic process that promotes social learning by collective inquiry and public deliberation.

The next three chapters discuss the encounter between science and politics in three substantive realms: natural resources management, global climate change, and risk assessment. Rather than address the same themes across each of the realms, each chapter raises new issues. For example, one of the emphases in Chapter 4 is the decline of the expert-based technocratic model of decision-making and the rise of adaptive management, ecosystem management, and community management as alternative models. Chapter 5 includes an exploration of the science and politics of uncertainty, while Chapter 6 includes an examination of the role of judgment in risk assessments (despite the assumption underlying much of the work in this area that risk is an objective property of the physical world).

The three concluding chapters make recommendations on how to more effectively integrate science into environmental policy. Chapter 7 argues in part for a need to recognize that the credibility of science is determined within society as a whole, rather than just within the community of scientists. Chapter 8 explores the relationship between democracy and science, and concludes with an argument for a deliberative democracy that conceives of scientists as one party contributing to a dialogue aimed at mutual learning. Chapter 9 is a brief conclusion that reiterates key points regarding the questions of how to evaluate science and how science should relate to societal context from a democratic point of view.

Although informed by philosophical debates about the character of science, Bocking emphasizes actual examples from the encounter between science and politics in his discussion. For this reason, this would be a good discussion textbook for a graduate or upper-division capstone course in natural science-based programs (e.g., wildlife biology, biological sciences). These programs typically rely on introductory courses in political and social science as the means of fulfilling educational requirements in these fields of knowledge. Yet, the type of civic and social knowledge covered in these introductory courses is divorced from the context in which it is relevant to students in the natural sciences. Bocking's book exposes these students to politics and society in a context relevant to their professional lives.

This strength, however, also represents a limitation of the volume. Chapters 4 through 6 explore different substantive contexts. This comes at the expense of examining a broader set of dimensions. For example, Chapter 4 begins with a discussion of the expert-based, technocratic decision-making philosophy that emerged in federal agencies at the start of the 20th century. However, it does not discuss the social context and concept of democracy that underlies this perspective. Such a discussion could help students understand the relationship between science and the evolving social context. Similarly, exploration of the role of the mass media in shaping interactions between science and environmental policy gets limited attention, as does the distinction between the realms of science and management and the contribution of social science to the development of environmental policy. Still, these subjects do receive some attention and any author must make difficult decisions about what to include and what to omit. Further, the breadth in terms of substantive realms will serve well those classes or readers interested in a discussion of the intersection of science and environmental policy that is not limited to a particular substantive realm.

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SCIENTISTS DEBATE GAIA: THE NEXT CENTURY. *Based on a conference held at the University of Valencia, Spain, 19–23 June 2000.*

Edited by Stephen H Schneider, James R Miller, Eileen Crist, and Penelope J Boston. Cambridge (Massachusetts): MIT Press. $50.00. xvii + 377 p; ill.; index. ISBN: 0-262-19498-8. 2004.

This volume contains 31 articles and four introductory pieces by more than 50 contributors. It is the product of the Second Chapman Conference held in 2000 to mark advances in Gaian theory. Introducing the book is an important preface by the editors, as well as statements by James Lovelock and Lynn Margulis on the state of the Gaian paradigm. A section on principles and processes (six chapters) tests the perimeters of the idea with new models. Part II, Earth History and Cycles (six chapters), explores the self-regulation function of life in the biosphere. The next part, Philosophy, History, and Human Dimensions of Gaia (seven chapters), examines the evolution of the Gaian paradigm. Part IV, Quantifying Gaia (seven chapters),

The Generation, Management, Migration, and Evaluation of Knowledge Within and Across Communities of Practice: An Annotated Bibliography

Statement of Purpose:

Knowledge comes in a variety of forms (e.g., wisdom, science, information, outformation (Ezrahi, 2004)) and resides in a variety of different institutional contexts (e.g., universities, medicine, law, administrative decision making). Across these institutional contexts, the generation, management, use and standards of evaluation for knowledge, though similar in some respects, also differs. Even within fields of practice, there are different subcultures that value different types of knowledge and have different validity standards (e.g., different views on the nature of science, the tension between evidence based medicine and traditional clinical practice).

Knowledge is power and political capital since it is often used to legitimize decisions due to its supposed superiority to values, emotions, and "politics". The generation, management, and use of knowledge are inextricably linked to institutions of democracy and different political configurations (Ezrahi, 2004). As knowledge is generated and transmitted across institutions and communities, it is contested both within and across communities. There is a growing body of literature within the realms of science, law, administrative practice, medicine, and media studies looking at these aspects of knowledge generation, transmittal, and use. The purpose of this paper is to provide an integrated overview exploring concepts related to knowledge generation, migration, and evaluation across these realms.

Table of Contents

The first section (Overview of Knowledge Related Themes) organizes references by the principle knowledge related theme that led to the decision to include the article in the bibliography. Within this section there are 4 tables, each reflecting an over-arching theme. Each table includes a brief (several sentence) description of the focus of the paper relative to the overarching theme. Additionally, table entries are grouped by subthemes within the overarching theme so that paper dealing with similar themes are grouped together. In the electronic version of the annotated bibliography, each entry in the table contains a hyperlink to a more extensive annotation. On occasion articles are listed in more than one table. The extended annotations are organized by primary discipline/field of origin as indicated by the other headings in the table of contents. The extended annotations are generally a series of quotes excerpted from the paper to convey the content. However, sometimes they are the actual abstract or my notes from reading.

Overview of Knowledge Related Themes

S01 Articles contrasting different types and/or standards of science/knowledge

General Discussions about Knowledge	*	Ezrahi 2004	Contrasts wisdom, (scientific) knowledge, information, and outformation as means of knowing and their implications for democracy and political configurations.
	*	Doremus 2004	Different standards for reliability, objectivity, and certainty are applied to scientific data in different contexts (research, courtroom, and regulatory).
		Jasanoff 2004	The field of science and technology studies has made considerable progress toward illuminating the relationship between scientific knowledge and political power. This book of edited chapters develops the theme of co-production, showing how scientific knowledge both embeds and is embedded in social identities, institutions, representations, and discourses.
		Cowan 1998	The contract between writer and audience (regarding truth, factuality) differs by genre. Explores different standards depending on the source/genre.
Administrative Practice	*	Hummel 1991	Some analytic scientists confuse two operations: the analytic operation of taking reality apart and the synthesizing operation of putting reality together. It is to the eternal credit of managers that they resist the importuning of scientists that managers need to get in line with analytic validity standards. Mangers are quite capable of judging what kind of knowledge is useful to them and of developing validity standards relevant to their world.
		Ivancevich et al. 2005	Some scientists and practitioners believe the academic knowledge base can be transferred and applied without jeopardizing the rigor and values of scientists. In examining the current landscape, it is reasonable to ask if there is any hope of closing the gap. Despite these points of tension brought about by different needs, values, training, education and experience, scientists and managers can still benefit from more collaboration.
Legal Practice and Science	*	Haack 2001	Legal efforts have often been based on false assumptions about science. A better understanding of scientific inquiry will reveal why it has proven so difficult to find: a legal form of words that will ensure only decent scientific evidence is admitted or a simple way to delegate responsibility to scientists themselves.
		Jasanoff 1995	The goal of the book is to: understand how the legal process mediates among conflicting knowledge claims, divergent underlying values, and competing views of expertise.
		Hacking 1999	In the science, officially, truth overrides every other virtue. In the law, officially, justice likewise overrides. That is neither to deny that justice should be tempered by mercy, nor that truth should be qualified by wisdom. On practical concerns, how should scientific information and advice be used in the law courts?

		Bohrer 1999	For at least twenty years there has been a significant debate about the difficulty of incorporating science into legal decision making. The authors' goal to reveal what lies at the core of the difficulty and which he believes accounts for so much of the current frustration over the role of science in the court room. The chapter is not about solutions to these issues, but rather an attempt to describe more fully the nature of the problem.
		Erickson & Simon 1998	Discusses the clash of cultures of law and social science. Central to this clash are differences in method and epistemology that feed the ongoing debate over the validity, neutrality, and objectivity of social science data and the role of statistics, certainty, and probability.
		Lempert 1986	We argue that court cases tend to be approached from either a shallow case logic (occurs when social/typical meanings satisfy whatever situational demands adjudication imposes) or a deep case logic (likely when the adjudicatory search is for an actor meaning, that is an understanding of the action from the actor's point of view). We note how different case logics produce different meanings of justice.
Medical Science vs Medical and Legal Practice		Shuman 2001	This article examines how the legal system has responded to that challenge of considering the science underlying claims of medical expertise; why the response has been more limited than many had expected; the implications of the legal system's approach to scrutiny of claims of medical expertise for the practice of science-based medical evidence; and, the central elements of any meaningful change in legal assessments of expertise in medicine.
		Eisenberg 2001	The author notes six different ways in which these tensions between legal and health care approaches to the concept of evidence were revealed: (1) population probabilities vs individual causation; (2) prehoc versus post hoc evidence; (3) differences in the way change/progress occurs; (4) differences in the way experts are used to adjudicate differences; (5) differences in rules of evidence; and (6) differences in who makes the decisions about validity of evidence. The basic tensions between these professional cultures will not readily dissipate.
		Mello & Brennan 2001	Problems occur when judges are faced with complex questions of scientific fact. A key assumption underlying many efforts seems to be that the gulf between law and science as disciplines is so vast that it can never fully be bridged, a perspective Eisenberg (2001) argues. The current authors take issue with some of the dichotomies Eisenberg sets forth.
		Mulrow & Lohr 2001	Developments in evidence based medicine are an aid, but not a panacea, for definitively establishing benefits and harms of medical care. The contributions that medical research evidence can make in any clinical or legal situation must be understood in a context in which judgment and values, understanding of probability, and tolerance for uncertainty all play a role.
		Rodwin 2001	Evidence-based medicine is portrayed as an alternative to medicine based on authority, tradition, and the physician's personal experience. When discussed, politics is portrayed as what evidence-based medicine will avoid. However, evidence is a tool for institutional control and policy argument. Promoting medical practice based on evidence will necessitate more, not less politics.
		Rosoff 2001	Article explores the politics of evidence based medicine and associated clinical practice guidelines.

		Greer 1999	Between medical science and medical practice, problems arise because the two activities have different goals, cultures, and operating codes. Efforts to internalize science create unique problems for both legal and medical practitioners on the one hand, and scientists on the other.
		Tanenbaum 1994	This small-scale ethnographic study evaluates claims that probabilistic knowledge will improve clinical practice. It finds that although they reason probabilistically in some instances, practicing physicians rely on personal experience over research data at times, and that doctors view outcomes research as useful but subject to social influences. The recent ascendancy of outcomes research is as much political as scientific, empowering the research community relative to practicing physicians, lending medical legitimacy to payer promulgated guidelines, and creates a false standard of medical certainty.
		Havighurst 2001	Overview for a special issue inspired by a concern that legal uses and interpretations of science-based medical evidence, particularly population studies and the findings of controlled clinical trials, may diverge substantially from that of researchers who produce it and from that of practitioners and health plans that use it in making clinical decisions and policies.
Science		Flyvbjerg, 2001	Book dealing with the philosophy of science
	*	Haack, 2003	Haack gives a "Critical Common-Sensist Account of science." Science is not sacred: it is thoroughly fallible, imperfect, uneven in its achievements, sometimes corrupt, and incomplete. In short, the sciences are not epistemologically privileged. They are, however, epistemologically distinguished. But distinction, unlike privilege, has to be earned; and the natural sciences have earned, not our uncritical deference, but our tempered respect.
		Giorgi 1997	A discussion of phenomenology that entails a general discussion of standards for science. In order to be scientific, knowledge must be (1) systematic, (2) methodical, (3) general, and (4) critical.
		Goodwin and Horowitz 2004	Some social scientists view qualitative sociology, in no uncertain terms, as methodologically and empirically "soft" and highly subjective, if not completely solipsistic. We believe that this view of qualitative sociology is badly mistaken. At its best, qualitative sociology can be very rigorous and "scientific" indeed.
	*	Daston 1992	This paper traces the rise and ascendancy of the meaning of 'aperspectival objectivity' since it dominates current usage. Aperspectival objectivity is both conceptually and historically distinct from the ontological objectivity that pursues the ultimate structure of reality and from the mechanical aspect of objectivity that forbids judgment and interpretation of scientific results. It became the creed of scientist, an ideal corresponding to the practice of impersonal communication.
		Fuchs 2000	Author argues that objectivity as a special kind of social and communicative "medium" that separates science from other modes of communicating.

	Hanna, 2004	This paper discusses different views of objectivity. It argues that science progresses when "higher levels of communicative discourse" are reached by transforming subjective judgments regarding the generation and reduction of data or the testing of theories into objective decision procedures that are automatic or mechanical.
	Porter 1992	Objectivity in science is a mechanism to exclude judgment. Nowhere in science is the preference for mechanical and objective reasoning over the communication of complex judgment more evident than in the use of statistical methods to analyze data.
	Rescher 1990	Although scientists may prefer simplicity (e.g., Occam's Razor, objectivity over subjectivity), nature may or may not. There are definite advantages to pursuing a scientific approach that emphasizes simplicity. Those who pursue a simplified reality will "outdistance the latter epistemologically" because of gains due to cognitive economy.
	Wackers 1992	Points out that arguments against concepts like relativism are often found to be persuasive because they seem to paint an unpleasant picture of the future (one in which much is beyond our direct control and which lacks standardization). Wackers goes on to discuss and illustrate situations in which scientific perspectives have been persuasive because of what they promise to do (e.g., objective knowledge, control) independent of whether they demonstrate any capacity to do so or not.
	Mishler 1990	Inquiry guided research refers to the body of research that shares an emphasis on the continuous process through which observations and interpretations shape and reshape each other. It entails the dialectic interplay of theory, methods, & findings over course of study. This feature marks departure from the dominant model of hypothesis testing experimentation.
	Tognetti 1999	Bateson argued for the addition of usefulness and relevance of results to decision making as quality criteria in post normal science. This implies inquiry into context at different levels of complexity. This in turn implies emphasis on processes that facilitate inclusion of diverse perspectives - which facilitates an understanding of relationships among different aspects of a problem. It also requires a reflexive approach to decision making.
	Grantham 2004	This paper considers the pursuit of unity in science. It argues for conceptualizing unity as "interconnection" (rather than reduction). According to this fields are unified to the extent that they are densely connected.
	Box 1992	Part of the early 90's debate in public administration about what research counts as science.
	Bailey 1992	Makes a case for the validity of case study method as part of the early '90's debate in public administration about what research counts as science.
	Addis and Podesta, 2005	The authors examine the epistemology of marketing and suggest it has partly diverted researchers' attention from the theory, and focused it mainly on the method: a distorted mechanism created to guarantee the scientific nature of the discipline by using scientific methods considered universal and immutable.

S03 Articles exploring the role of science/knowledge in decision making and politics

Media, Democracy, and Knowledge	*	Ezrahi 2004	Contrasts wisdom, (scientific) knowledge, information, and outformation as means of knowing and their implications for democracy and political configurations
	*	Graber 2004	Contrasts the "informed" citizen model and the "monitorial" citizen model and the role of the media as a means of mediating politics in complex modern societies.
		Graber 2003	Discusses the "hallowed belief" that democracy requires active citizens and news media that supply them with information they need to participate effectively in politics. Suggests neither group can fulfill the role expected of them under this model but that democracy can still persist as political culture may be more important that citizen wisdom or media excellence.
		Lange 1993	Bennett contended that the nature of contemporary political dramas threaten the vitality of democracy. If parties who create American political information campaigns are somehow locked into an inherently flawed system, we have indeed reached the crisis in political communication. This case study examines a resource conflict in which opposing parties rhetorical and communicative strategies reflect just such a self-perpetuating, negative, downward spiral logic of interaction.
Who/How to Judge Knowledge: Politics, Technocratic Wish, Practioners, or Negotiation		Revkin 2004	Marburger argues that when scientific information is flowing through government agencies, the executive branch has every right to sift for inconsistencies and adjust the tone to suit its policies, as long as the result remains factual. "Science has so many self-correcting aspects that I'm not really worried about these things." He acknowledged that environmental and medical issues, in particular, would continue to have a difficult time in the policy arena, because the science was fundamentally more murky than in, say, physics or chemistry.
		Houck 2003	Concludes with four cautionary notes for scientists regarding the interaction of science in development and execution of environmental policy: (1) beware of the lure of scientific management, (2) beware the lure of "good science," (3) beware the sources of funding, and (4) beware the lure of the safe, "apolitical" life.
		Dustin & Schneider 2005	The crux of the paper concerns the politicization of natural resource policy and ways in which research scientists tend to get caught up in it.
		IUFRO 2004	Summarizes a workshop, the goal of which is to explore strategies and mechanisms for improving communication between forest scientists and policy-makers so as to ensure that sound science is considered in the formulation of forest policies and on-the-ground forest management practices.
		Belkin 1997	Discusses the "technocratic wish" - an appeal to objective measures to resolve contentious issues and/or clothe their resolution as scientifically logical and natural; part of the debate between evidence based vs traditional clinical practice based medicine

		Hummel 1994	Empirical science in general, is for purposes of deciding on judgments "useless" to decision makers and "of no value in the judgment process.
		Pedynowski 2003	There can be no universal answer to 'which' knowledge framework is most suitable for decision making in all situations. This judgment of should be presented as a question of competing criteria for the validity in the eyes of those who wish to use the knowledge. There are clearly situations where the decision is not straight-forward. Still in these cases, the approach to the arbitration should be the elucidation of the methodologies of the knowledge production and the comparison of these to the goals purposes of the decision.
		Tognetti 1999	Bateson argued for the addition of usefulness and relevance of results to decision making as quality criteria in post normal science. This implies inquiry into context at different levels of complexity. This in turn implies emphasis on processes that facilitate inclusion of diverse perspectives - which facilitates an understanding of relationships among different aspects of a problem. It also requires a reflexive approach to decision making.
		Pasko 2002	Science is a tool of analysis. Science should be used only to form a broad knowledge base. It is not capable of choosing among alternatives that it helps to define. This requires combing scientific and logical reasoning with social values
		Matthews 1999	Recent authors have argued that practice guidelines in health policy are a contemporary manifestation of broader concerns about how objective decision procedures become politically legitimated. Author argues that the introduction of practice guidelines may promote the policy objective of cost effectiveness. However, their in actual court cases may be difficult because legal reasoning focuses on the particular facts in the case at hand rather than abstract decision procedures. Ongoing political negotiation will be necessary if the technocratic faith in practice guidelines is to become justified in reality.
		Jasanoff 1993	Social constructivism implies that processes fostering negotiation rather than confrontation are most likely to lead to acceptable consensus positions on scientific issues. According to this view, parties who participate in negotiating competing claims will sooner converge toward a shared cognitive position than those who remain outside the negotiation process.
		Cross 1992	Compromise and consensus are poor approaches to policy in science based matters.
		Webler & Tuler 1999	Success of policy making depends on fostering cooperation and collaboration among all interested parties. This paper views decision making as entailing two parts: analysis and deliberation. This perspective represents an advance over the view that policy making is an uneasy combination of science and politics. The paper presents a decision making framework appropriate where policy making occurs in a context involving complex value laden judgments under conditions of imperfect knowledge in the context of a democratic, highly litigious society.

Daubert Standards and Decision-making		King 1995	Daubert guidelines were modeled after scientific methodologies. These can and should be used to guide evaluations by administrative agencies.
		Madden 2003	Employing Daubert principles in judicial review is entirely appropriate within the administrative law scheme to ensure reasoned decision-making that is documented and substantiated, as well as to improve agency accountability. Additionally, it is a necessary reform to counterbalance agency bias, increase public trust, and create consistency in the federal judiciary's approach to NFMA analysis.
		Meyer 1999	Daubert and Joiner, have strengthened judges discretion as gatekeepers of scientific evidence. The application of Daubert has been inconsistent on where the border between scientific and nonscientific experts lies. This uncertainty threatens an important purpose of law, namely to provide a predictable outcome. It is important that judges, experts, and litigators better understand the strengths and limits of their own and each other's fields.
Courts, Disdretion, and Decision Making		Stevens 2002	Good science is the necessary foundation for resolving environmental conflicts. This can create a serous burden for government wildlife scientists, who must strive to put their personal values aside in the interest of producing the best objective science. Courts must find effective ways to ensure that wildlife science at the heart of these disputes remains objective and reliable. To do this successfully, courts must steer a careful course between respecting agency discretion and rigorously testing methodological assumptions underlying science.
		Morreim 2001	Recently there has been a marked interest in evidence-based medicine. This essay focuses on the following questions: how should courts respond to health plans' demands for evidence-based medicine; how should courts respond to plans' efforts to balance costs against benefits; how should courts screen testimony about physicians' alleged malpractice; what kinds and amounts of evidence should courts expect from litigants in medical cases. This move from empirical description into a normative discussion is important, because we cannot determine what courts ought to do simply by examining what they have done thus far.
		Rosenbloom 1997	The judiciary's appreciation for diversity and individuality is also ingrained in the case method and adversary proceeding. The judicial approach to decision making requires that an individual be allowed to state his or her own personal case. Social scientific generalizations may be used, but ultimately judicial decisions turn on the facts presented in idiographic circumstances sometimes affecting only one or a few people directly.
		Halpern 2001	analyzes the role of science in the legal decisions surrounding the Stellers and hypothesizes some effects of multi-statutory management in this case-study.

S04 Philosophy of Science articles calling for an approach that has parallels to critical pluralism

		Dupre 2002	Paper attacks the perennial philosophical and scientific quest for a simple and unified vision of the world … The only route to a deeper understanding of ourselves is through radical epistemological pluralism.

		Haack 2003	There can no more be rules for when a theory should be accepted and when rejected than there could be rules for when to ink in a crossword entry and when to rub it out; "the" best procedure is for different scientist, some bolder, some more cautious, to proceed differently.
		Harding 2000	There are many respects in which philosophies of science could encode democratic ideals, this chapter focuses on just one - the idea that the "universality ideal" is scientifically and political dysfunctional.
		Pedynowski 2003	There can be no universal answer to 'which' knowledge framework is most suitable for decision making in all situations. This judgment of should be presented as a question of competing criteria for the validity in the eyes of those who wish to use the knowledge. There are clearly situations where the decision is not straight-forward. Still in these cases, the approach to the arbitration should be the elucidation of the methodologies of the knowledge production and the comparison of these to the goals purposes of the decision.

S05 Articles linked to the idea of knowledge management

		Empson 2001	Broadly, there are two alternative perspectives on knowledge in organizations: "knowledge as an asset" versus "knowing as a process."
		Mishler 1990	Skilled research is a craft learned by apprenticeship to competent researchers by hands-on experience, and by continual practice. This knowledge can be accumulated in part through the development of exemplars. Learning from exemplars is a process contextually grounded in practice. An important task for less well-developed approaches to science is to develop a collection of relevant exemplars.

Administrative and Managerial Practice - Science

Bailey, M. T. (1992). Do physicists use case studies? Thoughts on public administration research. *Public Administration Review, 52*(1), 47-54.

Is the case study approach an appropriate methodology in public administration research? In contrast to the views of McCurdy and Cleary (1984), White (1986b), and others, Bailey posits that case study methods are central to the work of physics, chemistry, and other "hard" sciences which those critics seek to emulate in the study of public administration. She argues that properly structured case studies will live up to the scientific standards of rigor, including generalizability, transferability, and replicability. In addition, Bailey addresses the argument against the practitioner focused research by reasserting Dwight Waldo's contention that a bond exists between theory and practice in public administration.

Box, R. C. (1992). An examination of the debate over research in public administration. *Public Administration Review, 52*(1), 62-69.

Is the state of public administration research as gloomy as portrayed by critics? Richard Box reviews the Public Administration Review series that began with McCurdy and Cleary's (1984) piece on public administration research and offers a critique of the pessimistic assessment they provide. He challenges the assumptions made about the kind of research done in the "mainstream" social sciences which the series' authors seek to emulate, as well as the lace of reference to research in more practitioner oriented fields such as law, planning, etc. Box challenges the narrow view of "'scientific writing style" assumed by these critics as well as their contention that public administration research is not really addressing the

core issues of the field.

Cross, F. B. (1997). The consequences of consensus: Dangerous compromises of the Food Quality Protection Act. *Washington University Law Quarterly, 75*, 1155-1206.

While compromise obviously has some merit in disputes between competing public values, it is a poor policy for fundamentally science based matters. One does not try to establish the origins of humanity by crafting some compromise between creationism and evolution. Likewise, if the goal is to maximize protection of public health, compromise between interest groups is unlikely to yield the best policy.

Doremus, H. (2004). The purposes, effects, and future of the Endangered Species Act's best available science mandate. *Environmental Law, 34*, 397-450.

Both supporters and opponents of the ESA are intent on gaining the scientific high ground. In part, that desire undoubtedly stems from the Act itself, which requires that a number of key decisions rest upon the best available scientific information. But it goes deeper than that. Scientific victory seems to hold the key to political victory. Both sides seem convinced that public opinion turns on whether, and to what extent, science supports the decisions of the agencies responsible for implementing the ESA. .. As a society, we hunger for objective, rule-based decision making, especially when the decision pits human interests against those of another species. We worry that decisions lacking a firm, objective basis may be arbitrary, wholly "political," wholly dependent upon the whims of the particular decision maker, or made on the basis of improper motivations. We look to "science" to provide the objectivity we crave. The demand for strong scientific support of ESA decisions will only grow in the future, as the costs of conservation measures become higher and more apparent.

Science, however, is not as objective or neutral a basis for decisions as we might hope. In recent years, the uncertainties and gaps in the supposedly "scientific" decision making of the ESA have become increasingly apparent, and increasingly the source of controversy and contention. At the same time, several reviews have strongly endorsed the use of science in making decisions under the ESA. That juxtaposition suggests that different observers are looking for different things from the use of science in these decisions. The controversies raise important questions about how science is actually being used, what we are seeking from science, what we are actually getting from it, and what we should realistically expect. ...

This Article investigates the use and effects of science in ESA implementation. Section II begins with a description of the ESA's comprehensive "best available science" mandate. Section III provides a brief taxonomy of science explaining the different standards of reliability, objectivity, and certainty applied to scientific information in research, courtroom, and regulatory contexts. Section IV then analyzes at some length the possible intent and apparent consequences of that mandate as currently implemented. Finally, Section V offers several suggestions to improve the use of science in the ESA context. While there is little evidence that federal regulatory agencies are routinely or intentionally misusing science, those agencies are not always making the best use of science. They are not addressing openly the limitations of science and their treatment of uncertainty, they do not put enough emphasis on updating both science and the regulatory decisions that depend upon science, and their procedures are not calculated to build credibility. I offer some suggestions for improvement in each of these areas. Importantly, none of these suggestions require amendment of the ESA. That means they might realistically be tried even in the face of the legislative gridlock that has prevented ESA reauthorization since 1992.

Dustin, D. L., & Schneider, I. E. (2005). The science of politics/the politics of science: Examining the snowmobile controversy in Yellowstone National Park. *Environmental Management, 34*(6), 761-767.

The snowmobile controversy in Yellowstone National Park not only pits snowmobilers against environmentalists, but it also pits the Bush Administration against the Clinton Administration. Caught in the middle are the National Park Service, scores of natural and social scientists, and Yellowstone's permanent residents-the flora and fauna. The controversy's political aspects are the focus of this paper; specifically, the tenuous relationship among research scientists, whose job it is to inform management and policy decisions; politicians, whose job it is to formulate those same decisions in the public arena; and public land management agencies, whose job it is to implement the decisions. The crux of the paper concerns the politicization of natural resource policy and ways in which research scientists tend to get caught up in it. Lessons learned from this Yellowstone episode regarding the role of science in policy-making processes are also considered. Two recent federal court rulings shed additional light on the politics surrounding Yellowstone's snowmobile controversy, as does the importance of governmental checks and balances in resolving natural resource management disputes.

Hummel, R. P. (1994). Commentary. *Public Administration Review, 54*(3), 314.

Waldo's critique of social science focuses on its inability to help the judgment process on the part of "anyone who, honestly trying to face all acts, has a decision to make concerning human lives, values, and future."

Few practitioners in our field will disagree with this. After the best of scientific studies, the question still remains for the public service practitioner: How can I integrate what science has taken apart into my job that is to put things together?

A judgment must be made about the relevance of a piece of scientific research to a manager's or a worker's practical question at hand. In this judgment, science itself is not at all helpful; it was not designed to be. It was designed to take abstract observations and measurements from a complex and controllable world of the experiment of pure reason, and then report the results. How and whether these results can be integrated into practical judgments of what to do next is not told either by the scientific method, which is analytic not synthesizing, nor by empiricism's results. Waldo was right and remains right: not only the social science he refers to, but empirical science in general, is for purposes of deciding on judgments "useless" to decision makers and "of no value in the judgment process."

In this our serious Dwight has the support of serious philosophers like Immanuel Kant who distinguishes the functions of reason and science from that experience and practical judgment (Critique of Pure Reason). The great unresolved question remains: how to integrate the kind of knowledge that science can give with practical judgment about what the situation requires.

Hummel, R. P. (1991). Stories managers tell: Why they are as valid as science. *Public Administration Review, 51*(1), 31-41.

Nature of Knowledge and The Science Debates: Despite scientific critics who state what ails managers and public administration is that they are not scientific enough (Hummel 1991:31)... The question "Why can't we resolve the research issue in public administration?" has an embarrassingly simple answer. It is because some analytic scientists confuse two operations: the analytic operation of taking reality apart and the synthesizing operation of putting reality together . ... The manager's world seems to be founded on synthesis not analysis (Hummel, 1991:33). ... It is to the eternal credit of managers that despite the pressures of a scientific culture and a rationalist/scientist education, they have remained realistic. ... they resist the importuning of scientists that managers need to get in line with analytic validity standards when mangers are well and appropriately engaged in their prior task of putting and keeping the world of work together. (Hummel 1991:35) ... in-depth conversations with managers show that they are quite capable

of defining their reality, judging what kind of knowledge is useful to them, and developing validity standards relevant to their world. (Hummel 1991:32)

Intersubjectivity vs. Objectivity: Managers question the need for all-pervasive objectivity; to them a reality is constituted not by consensus of all imaginable detached observers but by the present community of those involved in the problem who must be brought along to constitute a solution. ... Managers find themselves in social situations in which they do not have the power to set up a way of looking at a problem so that the problem will appear the same to all involved. ... The manager has some authority and some discretion. But the institutional framework does not provide the total latitude necessary to permit her the scientific freedom to "set up" a way of looking at the problematic event that would, according to the scientific analytic method, guarantee one way of seeing it. ... The point here is that a problem in mgmt often arises because different people involved in a situation cannot agree. This is not [necessarily] because they are people of ill will, irrational, or anti-scientific, but because they are so placed in the organization that their roles give them specific perspectives that they are not necessarily compatible ... In intersubjectivity, all agree to respect each other's definition of the problem and, by respecting this, puzzle out a synthesis that leads to a solution. (Hummel 1991:33)

Uniqueness vs Replicability: ... even where there are rules summarizing past experience, a judgment still must be made as to whether those rules apply in this situation. (Hummel 1991:34) Science assumes that aggregate data analysis produces rules governing a type of event. ... Managers question the relevance of the analytic scientific tenet that experiences pile up into an aggregate about which rules can be formed; to the manager this still leaves the problem of judging whether a rule about by-gone experiences applies to a new situation at hand. They question the separation of reality and observer. (Hummel 1991:31) ... Even if an event ... should repeat itself, the manager would want still have to make a judgment as to the degree to which past scientifically established variables could be used to affect the situation today. An astute manager might ask: If the problem is patterned enough for science to study it, what is it doing on my desk? Clearly for patterned problems there are organizational routines that can be installed to handle them. ... Even if a general pattern of scientific findings is available for this kind of research elsewhere, the manager must still make a judgment as to how the general and repeated pattern of the past fits this event. ... Grant for the moment that manager's knowledge is intersubjective rather than objective when it comes to problem definition. (Hummel 1991:34)

Validity Standards for Synthesis: The question remains, are there validity standards for synthesis? (Hummel 1991:36) ... Contrary to the analytic scientist who takes for granted the existence of a shared world view that has relevance to all who use his approach, the manager listening to a story is concerned w/ the prior problem of establishing the relevance of the world [in the story] to his own world and his interests in it. (Hummel 1991:37) ... The manager [in an example Hummel discusses] simply did not believe the engineers story, so scientific analysis could find no place in the construction of reality. (Hummel 1991:39) ... The validity the listener seeks is a structural validity. ... the structures of the storied world when compared to the structure of one's own. (Hummel 1991:38) ... [the] Criteria of coherence: whether it's credible in terms of your previous experience, whether it's comparable to other experience... (Hummel 1991:39) ... But ultimately what managers must judge is not what has been scientifically calculated in the past or even what trends the computer can project into the future. They must judge whether the data of science and rational calculation fit into a future no one has yet seen. ... "in a cabinet meeting. Sure we hand the [scientific] reports around, but then we have to see if they fit with what we can do" (Secretary of State, 1989 in Hummel 1991:39).

Ivancevich, J. M., Duening, T. N., & Lidwell, W. (2005). Bridging the manager-organizational scientist collaboration gap. *Organizational Dynamics, 34*(2), 103-117.

After over 80 years of accumulating organizational science research, there is now a body of knowledge that can be applied in practice. (103) Some scientists and practitioners believe the academic knowledge base can be transferred and applied without jeopardizing the rigor and values of scientists. Transferring knowledge, however, is a major task that needs to be undertaken by collaborating scientists and practitioners who filter, critique, and determine which research has practical utility, what problems still need to be examined, what research designs are needed, and where research can be conducted without obstruction or restrictions. ... Logic suggests that practitioners are most likely to seek out scientists when they face the most intractable problems, primarily because of the difficulty of solving them by themselves. If organizational scientists can illustrate the value and usefulness of their research, practicing managers are more likely to want to interact more frequently and to extend invitations to visit and observe organizational practices. ... If organizational scientists continue to provide only research-based findings and insights that are complex, incorporate obtuse constructs, or use sophisticated designs, practicing managers are unlikely to heed research findings or call for additional organizational research. (104)

In examining the current landscape, it is reasonable to ask if there is any hope of closing the M-OS gap. We propose that the gap can be closed by more collaborative work between managers and organizational scientists. (105) Collaboration means working together on projects, participating in discussion groups analyzing problems, issues, and trends, jointly exploring unique concerns of managers and scientists, and creatively developing projects and programs to improve the contributions of scientists and managers to society and its institutions. ... The development of a deeper, more meaningful M-OS collaborative relationship raises a number of issues. Different approaches to studying organizations through research can range from a theory orientation to an action-oriented perspective. Issues of rigor versus relevance, neutrality versus immersion, generalization versus specific, observing versus participating, measurement validity versus experiential, insider versus outsider, and context-free versus context embedded continue to be debated. Each of these scientist-versus-practitioner issues will likely remain as tension points. Despite these points of tension brought about by different needs, values, training, education and experience, scientists and managers can still benefit from more collaboration.

In traditional professions such as medicine, engineering, and law, there is more of a connection and interaction between ongoing research, practice, and education. (106) In medicine, it's not uncommon for physicians (practitioners) to be involved in ongoing research. In engineering, many professional engineers regularly undertake original research and publish their findings in practitioneroriented publications such as IEEE Transactions. Lawyers publish extensive legal reviews and conduct "moot court" clinics (e.g., simulations) to examine the effects and implications of various courtroom techniques, such as presentations of opening and closing arguments, introducing and using expert witnesses, explaining exhibits, and jury selection in law schools.

A more professional and scientifically based approach to management problems and decisions would be a positive step for employees, organizations, and society. A possible starting point is for managers and scientists to collaboratively identify and prepare an initial set of guidelines (e.g., prescriptive actions) that managers can apply. A sample of the type of guideline that might be included is displayed in Fig. 1. (107) This guideline on decision-making is stated in a prescriptive format instructing that an "acknowledged expert" should be used. The rationale and brief presentation of supporting evidence is provided. An emphasis is made here on the importance of clarity and conciseness. In reality, only a concise, representative set of the supporting evidence should be provided, because, in some cases, pages of reference notations could be offered for each prescription.

Webler, T., & Tuler, S. (1999). Integrating technical analysis with deliberation in regional watershed management planning. *Policy Studies Journal, 27*(3), 530-543.

Success of policy making depends on fostering cooperation and collaboration among all interested and

affected parties (530). This requires an understanding of the theory and principles behind the decision making processes. The National Resource Council (1996) identifies 2 parts: analysis and deliberation. Successful policy making requires finding the right combination of the two at every step in the policy process. This perspective represents an advance over the view that policy making is an uneasy combination of science and politics (531). The framework discussed in this paper very appropriate where: policy making occurs in a context involving "complex value laden judgments under conditions of imperfect knowledge in the context of a democratic, highly litigious society."

Distinction between Analysis and Deliberation - Analysis = systematic ways of gathering and integrating data - overarching principle is that results validated through systematic studies. Do not equate science with analysis. Deliberation, for example is informed by social science. Both are ways of making sense of the world. Deliberation = people confer, exchange views, consider evidence, reflect on matters of mutual interest, negotiate, and attempt to persuade each other. Thus deliberation is done by scientists as well as decision makers.

Clinical Practice - Medical Science

Belkin, G. S. (1997). The technocratic wish: Making sense and finding power in the "managed" medical market place. *Journal of Health Politics, Policy and Law, 22*(2), 509-532.

Enormous changes have recently swept through the organization and delivery of medical care. Scholars and students of the health care system and its politics try to make sense of the shift in power to identify and allocate needed resources away from physicians and toward corporate firms. I suggest that we cannot understand managed care unless we understand its power as at least substantially due to its reliance on a claim to be better science. In this way, managed care needs to be placed within an analytic historical tradition that is concerned with how accounts of scientific objectivity became convincing and support (and are confirmed as scientific by) social and political objectives. In this way, managed care reflects what I call the technocratic wish: an appeal to objective measures to resolve contentious issues and/or clothe their resolution as scientifically logical and natural.

Eisenberg, J. M. (2001). What does evidence mean? Can the law and medicine be reconciled? *Journal of Health Politics, Policy and Law, 26*(369-381).

Popular attention has focused of late on the role of evidence in health care. Physicians have been encouraged to practice "evidence-based medicine," so that their clinical decisions would be based upon a foundation of solid science, especially using research that has applied rigorous epidemiologic methods and has been published in peer-reviewed journals. Evidence-based medicine involves increased reliance on formal, systematic analysis and synthesis of the research literature to determine clinical effectiveness. It challenges consensus-based judgments and applies critical assessment of the available research to decide if there is methodologically sound evidence that the outcomes of a clinical option are favorable, and it identifies types of patients for whom the service is most effective.

The response of some clinicians has been gratitude for the recognition, implicit in evidence-based medicine, that the everyday practice of clinical care can be an intellectually rigorous undertaking. Others have responded less gently, asking, in essence, "So what have I been practicing, magic?" Indeed, there is sufficient evidence to suggest that most clinicians' practices do not reflect the principles of evidence-based medicine but rather are based upon tradition, their most recent experience, what they learned years ago in medical school, or what they have heard from their friends.

Different definitions of evidence in the healthcare and legal communities converge and conflict in several ways. In addition to deciding what evidence should be admitted, there is the challenge of determining

how the evidence should be weighed in driving a decision. Scholars seek to reconcile evidence that is probabilistic in health care with evidence that is "without a reasonable doubt" in criminal law or the "preponderance" of evidence in civil cases.

During the course of the workshop, I noted six different ways in which these tensions between legal and health care approaches to the concept of evidence were revealed: (1) population probabilities vs individual causation; (2) prehoc versus post hoc evidence; (3) differences in the way change/progress occurs; (4) differences in the way experts are used to adjudicate differences; (5) differences in rules of evidence; and (6) differences in who makes the decisions about validity of evidence. The basic tensions between these professional cultures will not readily dissipate.

Havighurst, C. C. (2001). Evidence: Its meanings in health care and in law: Summary of the 10 April 2000 IOM and AHRQ Workshop. *Journal of Health Politics, Policy and Law, 26*(2), http://www.dukeupress.edu/jhppl/.

In April 2000 the Institute of Medicine (IOM) and the Agency for Healthcare Research and Quality (AHRQ) jointly hosted a one-day workshop to explore an intriguing and important intersection of medicine and law: the courtroom presentation of science-based medical evidence and expertise. This workshop was inspired by a concern that legal uses and interpretations of science-based medical evidence, particularly population studies and the findings of controlled clinical trials, may diverge substantially from the uses and interpretation of that evidence by the medical and health care researchers who produce it and of the practitioners and health plans that use it in making clinical decisions and policies.

Recognizing that a preliminary discussion among professions was needed even to describe the nature of their differences, the IOM and AHRQ, at the instigation of John M. Eisenberg, director of AHRQ, convened about twenty clinicians, epidemiologists, health services researchers, health plan executives, practicing and academic lawyers, jurists, and social scientists in the field of legal medicine (see appendix for participants). Participants and presenters were asked to formulate empirical research questions concerning both evidence-based medicine (EBM) and judicial practices that might increase familiarity with, and therefore promote greater reliance on, the use of science-based medical evidence by the courts. Workshop participants were further asked to identify policy issues relating to the application of evidence-based medical findings that were emerging in the context of congressional consideration of patient protection legislation and reform of health plan liability law.

The four background papers commissioned for this workshop provided the participants with a common frame of reference for the issues to be addressed during the day. These papers were the first drafts of the authors' articles in this special issue. The authors were variously asked to address the following questions:

(1) What do physicians take to be evidence that justifies their practices and treatment decisions, and how has this understanding changed over time?
(2) To what extent has EBM affected the practice of medicine?
(3) What kinds of questions do rules of evidence allow medical experts to address in the courts?
(4) How do judges and juries understand and weigh scientific claims about the outcomes and efficacy of particular medical practices?
(5) What impact have recent Supreme Court decisions regarding the role of the judge in qualifying expert witnesses and screening scientific and technical evidence for presentation to juries had on malpractice cases and health plan coverage disputes?
(6) How are courts likely to deal with science-based medical evidence in cases involving health plan coverage disputes and medical necessity determinations under proposed liability reforms?
(7) What is the place of cost-effectiveness and cost-benefit analysis in health plan coverage policies, and

how will courts consider coverage choices based on these kinds of analyses?
(8) How can those involved in developing the evidence base for medical practice most effectively present this information in legal settings?

Matthews, J. R. (1999). Practice guidelines and tort reform: The legal system confronts the technocratic wish. *Journal of Health Politics, Policy and Law, 24*(2), 275-304.

Recent scholarly writing has argued that the advent of managed care within the health policy arena can be seen as a contemporary manifestation of a broader set of concerns focusing on how objective decision procedures become politically legitimated - what one recent commentator has characterized as a faith in the technocratic wish. In the 1990's, this faith in objective decision procedures has manifested itself through the emergence of outcomes assessment and the development of practice guidelines. Notably, a few states have sought to couple the practice guidelines movement with tort reform by enacting demonstration projects permitting physicians to introduce new evidence that they followed practice guidelines as an affirmative defense. In this article, I argue that even though the introduction of practice guidelines may promote the policy objective of cost effectiveness in the delivery of health care services, their use to establish culpability in actual cases may be more difficult because the structure of legal reasoning focuses on the particular facts in the case at hand rather than appealing to abstract decision procedures. By highlighting the potential difficulties of invoking practice guidelines in the adjudication of actual malpractice cases, I demonstrate how a process of ongoing political negotiation will be necessary if the technocratic faith in practice guidelines is to become justified in reality.

Mello, M. M., & Brennan, T. A. (2001). Demystifying the law/science disconnect. *Journal of Health Politics, Policy and Law, 26*(2), http://www.ahrq.gov/clinic/jhppl/mello.htm#bib5.

As the symposium in this issue of the Journal of Health Politics, Policy and Law makes clear, there remains a great deal of controversy and uncertainty surrounding the treatment of scientific evidence by the courts. Many of the issues are not new: they began to be identified in the legal literature in the mid-1980s (Brennan and Carter 1985). The fifteen years of struggle by courts and legal commentators have not, however, resolved the problems that occur when judges are faced with complex questions of scientific fact. To use a medical metaphor, we have been struggling to make a diagnosis-to clearly articulate the nature of these problems, with a view to proposing therapeutic reforms. A key assumption underlying these diagnostic efforts seems to be that the gulf between law and science as disciplines is so vast that it can never fully be bridged. This is a central argument of the crisp article by John M. Eisenberg, the director of the Agency for Healthcare Research and Quality (AHRQ). Eisenberg identifies a series of differences between law and medicine and concludes that given these differences in institutional views, judges' problems with medical and scientific issues are not only understandable but inevitable. One could, we think, respectfully take issue with some of the dichotomies Eisenberg sets forth, and we do so below.

Morreim, E. H. (2001). Commentary: From the clinics to the Courts: The role science should play in litigating medical care. *Journal of Health Politics, Policy and Law, 26*(2), http://www.dukeupress.edu/jhppl/.

Throughout this collection of essays, the Institute of Medicine and the Agency for Healthcare Research and Quality have identified an issue whose importance and nuances we are only beginning to appreciate. Although medicine has long claimed to be rooted in science, actual clinical care has often had only a limited scientific basis, resulting in inexplicably wide variations of care (Wennberg 1996). The past few years have witnessed a marked interest in evidence-based medicine (EBM), stemming from several concerns. First, decades of double-digit health care inflation led to a recognition that enormous amounts of money have been wasted on interventions with little proven value.... Second, the more forward-

looking health plans aim, not just to cut costs, but to render care more rational. ... Third, some plans' rather drastic cost-cutting measures have occasioned concerns that basic quality of care is suffering. ... Finally, beyond simply avoiding poor-quality care, many purchasers, particularly large employers, seek affirmative value for their dollars. ... the essays in this collection explore important questions about the ways in which courts' quest for more and better evidence in other contexts may dovetail with health plans', providers', and purchasers' demands that clinical practices, and the guidelines sometimes imposed on them, reflect an adequate scientific foundation. Mainly the questions explored in this collection are empirical: how has evidence-based medicine in fact affected clinical practice; how do judges understand and weigh scientific claims; how will courts address evidence-based medicine and cost-effectiveness analysis in coverage disputes; and so forth... This excellent foundation makes it possible to launch into some related normative (i.e., evaluative) issues that will be the focus of this essay: how should courts respond to health plans' demands for evidence-based medicine; how should courts respond to plans' efforts to balance costs against benefits; how should courts screen testimony about physicians' alleged malpractice; what kinds and amounts of evidence should courts expect from litigants in medical cases. Thus this commentary represents, not so much a reflection on the core essays, as an exploration of some of the further issues those writings have prompted. This move from empirical description into a normative discussion is important, because we cannot determine what courts ought to do simply by examining what they have done thus far. ... Accordingly, it is appropriate to consider carefully what the approach of the courts ought to be regarding the uses of scientific evidence in litigation regarding health plans and providers alike. Health plans will be considered first, then physicians.

Mulrow, C. D., & Lohr, K. N. (2001). Proof and policy from medical research evidence. *Journal of Health Politics, Policy and Law, 26*(2), 249-266.

When judging the benefits and harms of health care and predicting patient prognosis, clinicians, researchers, and others must consider many types of evidence. Medical research evidence is part of the required knowledge base, and practitioners of evidence-based medicine must attempt to integrate the best available clinical evidence from systematic research with health professionals' expertise and patients' rights to be informed about diagnostic and therapeutic options available to them. Judging what constitutes sound evidence can be difficult because of, among other things, the sheer quantity, diversity, and complexity of medical evidence available today; the various scientific methods that have been advanced for assembling, evaluating, and interpreting such information; and the guides for applying medical research evidence to individual patients' situations. Recommendations based on sound research can then be brought forward as either guidelines or standards, and criteria exist by which valid guidelines and standards can be developed and promulgated. Nonetheless, gaps and deficiencies exist in current guidelines and in the methods for finding and synthesizing evidence. Interpreting and judging medical research involves subjective, not solely explicit, processes. Thus, developments in evidence based medicine are an aid, but not a panacea, for definitively establishing benefits and harms of medical care, and the contributions that medical research evidence can make in any clinical or legal situation must be understood in a context in which judgment and values, understanding of probability, and tolerance for uncertainty all play a role.

Rodwin, M. A. (2001). The politics of evidence-based medicine. *Journal of Health Politics, Policy and Law, 26*, 439-446.

The impetus for these essays on evidence in medicine and law is commonly called evidence-based medicine: the movement to evaluate the safety, effectiveness, and cost of medical practices using tools from science and social science and to base clinical practice on such knowledge. Evidence-based medicine is portrayed as an alternative to medicine based on authority, tradition, and the physician's personal experience. The role of politics is rarely mentioned. When discussed, politics is portrayed as what evidence-based medicine will avoid. Rational evaluation of evidence plays an important role in

medicine. However, it is not an alternative to medical politics. Rather, evidence is a tool for institutional control and policy argument. Today evidence-based medicine is used to oversee individual physicians and the practice of medicine. It thus helps to alter the balance of power among doctors, payers, and patients. Changing medical practice requires the development of political, legal, and medical institutions that oversee medical care. Promoting medical practice based on evidence will therefore necessitate more, not less politics.

Rosoff, A. J. (2001). Evidence-based medicine and the law: The courts confront clinical practice guidelines. *Journal of Health Politics, Policy and Law, 26*, 327-368.

This article examines how courts are likely to apply evidence-based medicine, and particularly clinical practice guidelines (CPGs), in healthcare litigation involving quality-of-care and entitlement-to-benefits (coverage) claims. Exploring the "politics" of the current situation, it observes that, just as clinicians have been reluctant to use CPGs in practice, courts have been, and likely will continue to be, slow to apply them in deciding cases. The article analyzes extant and proposed statutory approaches to legitimizing and promoting courts' use of CPGs. It concludes by renewing the author's earlier and controversial proposal to establish a voluntary federal program for certifying guidelines and directing courts to give certified CPGs greater weight in healthcare litigation.

Shuman, D. W. (2001). Expertise in law, medicine, and health care. *Journal of Health Politics, Policy and Law, 26*(2), http://www.ahrq.gov/clinic/jhppl/.

As the practice of science-based medical evidence has challenged the medical profession to consider the scientific bases for its methods and procedures, on a seemingly parallel path, the United States Supreme Court's 1993 decision in Daubert v. Merrell Dow Pharmaceuticals has challenged the legal system to consider the science underlying claims of medical expertise. This article examines how the legal system has responded to that challenge and why the response has been more limited than many had expected; the implications of the legal system's approach to scrutiny of claims of medical expertise for the practice of science-based medical evidence; and, the central elements of any meaningful change in legal assessments of expertise in medicine and health care.

Tanenbaum, S. J. (1994). Knowing and acting in medical practice: The epistemological politics of outcomes research. *Journal of Health Politics, Policy and Law, 19*(1), 27-44.

Recent health care policymaking favors outcomes research as a response to the putative ineffectiveness, as well as the undeniable expense of American medicine. This small-scale ethnographic study conducted in a department of internal medicine evaluates claims that probabilistic knowledge will improve clinical practice. It finds that physicians are primarily determinists and that although they reason probabilistically in some instances, they rely on personal experience over research data at these times; that doctors view outcomes research as useful but not definitive and in no way immune to the social influences on medical knowledge generally; and that their mix of determinism and probabilism is well suited to the nature of medical work. The recent ascendancy of outcomes research is as much political as scientific, empowering the research community relative to practicing physicians, lending medical legitimacy to payer promulgated guidelines, and creating additional clinical work around a false standard of medical certainty.

Legal Practice - Science

Bohrer, R. A. (1999). The fundamental differences between science and law. In C. Meyer (Ed.), *Expert Witnessing: Explaining and Understanding Science* (pp. 41-49). Boca Raton, FL: CRC Press.

For at least twenty years there has been a significant debate about the difficulty of incorporating science into legal decision making and, more recently, that difficulty has become part of the very heated debate over the impact of tort litigation ... on our economy and society. My ... goal for this chapter is to try to reveal something of what lies at the core of the difficulty and that which I believe accounts for so much of the current frustration over the role of science in the court room. ... The title ... is an attempt to convey that this chapter is not about solutions to these issues, but rather an attempt to describe more fully the nature of the problem. The first section ... will describe the three basic differences between the world of science and the world of law: science is digital, replicable/general, and objective/universal; law is analogical, unpredictable/particular. and normative/contingent. Part II provides examples of scientific and legal reasoning.... Part III concludes that potential differences between science and law require a distortion of the concept of legal causation, which is traditionally individual and particular, and is an essential element of our sense of justice and fairness.

Erickson, R. J., & Simon, R. J. (1998). *The Use of Social Science Data in Supreme Court Decisions.* Urbana, IL: University Press.

The breadth and complexity of issues surrounding the Supreme Court's use of social science data in making its decisions stem from the clash of cultures of law and social science. Central to this clash are differences in method and epistemology that feed the ongoing debate over the validity, neutrality, and objectivity of social science data and the role of statistics, certainty, and probability. ... Scholars from Jurgen Habermas to Richard Posner have predicted that the crisis of modern legal culture will lead courts to give social science a more prominent role in formation of legal policy; that if precedent and judicial intuition cannot supply a clear answer to a legal problem, perhaps empirical social science can. ... The extent to which this view is shared within the halls of justice or the social science academy is the focus of this section, beginning with the clash of the methodologies, the nature and ends of the truth seeking processes, of each community.

Greer, A. L. (1999). The end of splendid isolation: Tensions between science and practice. In C. Meyer (Ed.), *Expert Witnessing: Explaining and Understanding Science* (pp. 51-65). Boca Raton, FL: CRC Press.

It is a tribute to the power and charm of science that it has become a consistent and important reference point for far older institutions, including medicine and, in many of its concerns, the law. Legitimate decision-making in these latter domains now requires they incorporate scientific findings. The means to do this, however, remain controversial. That medicine does not correspond more closely to science has become a major concern of policy-makers, educators, managers, and payers of health care. All are investing immense energy in efforts to achieve closer conformity. In law, argument surrounding the value of scientific testimony in court proceedings has frayed nerves and led the US Supreme Court to accord trial judges wide latitude in deciding not only whether such testimony is relevant but to employ legal reasoning to decide whether it is reliable. My focus is on the struggle over the role of science in medicine with the thought that analogous professional difficulties may exist in the judicial process. Between medical science and medical practice, problems arise because the two activities have different goals, cultures, and operating codes. ... The efforts of medicine and the law to internalize science create unique problems for both legal and medical practitioners on the one hand, and scientists on the other.

Hacking, I. (1999). The time frame problem: The law, social construction, and the sciences. *The Social Science Journal, 36*(4), 563-573.

Among our official noblest virtues are truth and justice. In the sciences, officially, truth overrides every other virtue. In the law, officially, justice likewise overrides. That is neither to deny that justice should be tempered by mercy, nor that truth should be qualified by wisdom. Any discussion of law and the sciences

is potentially high philosophy, the meeting ground of truth and justice. But there are also more practical concerns. How should scientific information and advice be used in the law courts? There is now a rather staid but thoroughly solid assessment of the current state of play in the United States Federal Court systems, Foster and Huber's Judging Science: Scientific Knowledge and the Federal Courts (1997). Far more challenging to the philosopher, the scientist and the student of jurisprudence is Sheila Jasanoff's Science at the Bar (1995) because she enters current debates as a professed social constructionist, with a distinguished career of investigating science and public policy (Jasanoff, 1986, 1990).

Halpern, M. (2001). Stellar seal lions: The effects of multi-statute administration on the role of science in environmental management. *Journal of Environmental Law & Policy, 19*, 449-.

This paper examines the effects of the diversity of the NMFS's statutory responsibilities on the use of science in protecting the Steller. This introductory section continues with descriptions of the Steller, the fish, and the Alaskan Fisheries. Section two details the legal history, statute by statute, of the Steller and the Alaskan Fisheries and then briefly summarizes important events and decisions in an integrated time line. Section three analyzes the role of science in the legal decisions surrounding the Stellers and hypothesizes some effects of multi-statutory management in this case-study. The final section offers a brief policy analysis of those possible effects.

Houck, O. A. (2003). Tales from a troubled marriage: Science and law in environmental policy. *Tulane Environmental Law Journal, 17*, 163-175.

With such power and so much riding on the opinions of scientists, however, four notes of caution are in order. The first is to beware the lure of a return to "scientific management." The technology standards that brought environmental programs out of their stalemate toward success were criticized from day one, and remain criticized today, as "arbitrary," "one size fits all," "inflexible," and "treatment for treatment's sake," outmoded in today's world. What we need, goes the refrain, is "iterative," "impact-based," "localized" management focused on the scientifically determined needs of this river, that airshed, this manufacturing plant, or that community. It sounds as attractive and rational as it did forty years ago, but we have tried that for decades and failed. ... On the natural resources side of the ledger, the most abused concept in public lands management is "multiple use" and the most obeyed is the no-jeopardy standard of the ESA. One is a Rorschach blot; the other is law. The second caution is the lure of "good science." Every lawyer knows what "good science" is: the science that supports his or her case. All of the other science is bad. If you are opposed to something, be it the control of dioxins, global warming, or obesity, the science is never good enough. ... Adding it up, however, most junk science has come from boosters and developers and has erred on the side of unreasonable optimism. When, on the other hand, scientists have said that the ozone layer was thinning, the planet warming, and the fishery disappearing, they were usually ahead of their time, vilified, and on target. See the life and death of Rachel Carson. With this understanding as background, we see today, in the name of "good science," a proposal for "peer review" of all science-based agency decisions. The primary targets are decisions made by the Environmental Protection Agency (EPA) and the Department of the Interior. If the EPA proposes an environmentally protective action, it will likely be stalled for lack of consensus among "independent" peers. More studies will be commissioned, years will pass. Administrations will change. The opponents win. If, on the other hand, the EPA decides that TCE does not pose a significant risk to human health, or the Department of the Interior decides not to protect the Alabama beach mouse as an endangered species, there is no peer review, because no action is being proposed. What you have, then, is a knife that cuts only one way: against environmental protection. All in the name of "good science." Beware of being so used. The third caution is the lure of money, which works like the pull of the moon. One knows where lawyers are coming from; they speak for their clients. For whom does the scientist speak? Apparently truth and wisdom, but who pays for their work? Most academics in the sciences receive their salaries and technical support through grants and outside funding, nearly a third of it from industry. Their promotions and

tenure are based on the amounts of money they bring in. In 1998, the New England Journal of Medicine published an article with the unremarkable but statistically documented conclusion that there was a "significant difference" between the opinions of scientists who received corporate funding and those who did not, on the very same issues. Hearing this, do we fall over with surprise? To put it crudely, money talks, and among scientists, the money is too often hidden. Even a scientist's conclusions can be hidden if they are unwelcome to the sponsors. On important public issues, the public never knows. A final caution is the lure of the "safe" life, the apolitical life, free from the application of what scientists know to the issues around them. One must respect anyone's liberty to choose to be a player or not, and the additional need of the profession for the appearance, and fact, of objectivity. The question is, notwithstanding: given the pressure of environmental issues today and their dependence on science, can scientists afford to sit it out? As we speak, an increasing number of scientists are being pulled off of studies, sanctioned, and even dismissed for conclusions that contradict the ideology of their bosses. This question does not concern who pays for what conclusions. It concerns a duty to act and to defend your own.

Jasanoff, S. (1993). Procedural choices in regulatory science. *Risk, Health, Safety, and Evironment, 4*, http://www.piercelaw.edu/risk/vol4/spring/jasanoff.htm.

I will argue generally that adversarial procedures like the science court are less effective in achieving regulatory objectives than procedures that are more sensitive to the distinctive characteristics of regulatory science.

Social construction theory - advocates of social construction do not insist that ideological differences among experts are the sole determinant of variations in the interpretation of data. Evidence from social studies of science suggests, instead, that expert disputes can arise out of "honest" differences linked to disciplinary training, institutional affiliation, or professional status. For example, molecular biologists, toxicologists and epidemiologists may differ in their definitions of what constitutes an adequately controlled experiment These findings have important implications for science in the policy process, for they lead us to question popularly held beliefs about the definition of "good science." From the social constructivist vantage point, however, the creation of scientific knowledge is much less objective and methodologically watertight. "Truth" emerges not because nature, when interrogated by the scientific method, unambiguously reveals the answers, but because discipline-based scientists agree, through complex processes of negotiation and compromise, how they should choose among different possible readings of observations and experiments.

One does not have to believe rigidly in the constructivist account of science or adopt the most radical form of ontological skepticism to conclude that regulatory science is particularly susceptible to divergent, socially conditioned interpretations Academic research science, as practiced in university laboratories, tends to be conducted in environments of reasonably strong consensus, governed by established paradigms and relatively uncontested methodological and quality control standards. In regulatory science, by contrast, standards for assessing quality tend to be more fluid, controversial and sensitive to political factors. Important studies often straddle disciplinary boundaries, so that clearcut assessment standards are hard to identify.

The theory of social constructivism implies that processes fostering negotiation rather than confrontation are most likely to lead to acceptable consensus positions on scientific issues. According to this view, parties who participate in negotiating competing claims will sooner converge toward a shared cognitive position than those who remain outside the negotiation process.

Conclusion - Case studies suggest that the legitimacy of scientific assessments in a policy setting can be enhanced through procedures that stress negotiation and compromise, rather than adversarial conflict, among interested parties. The constructivist viewpoint implies ... that claims concerning regulatory

science can be made more credible to both lay and expert audiences if the independent scientific community engages with other interests ... in a process of mutual accommodation. When outside scientists are poised adversarially in relation to the agency, rifts may develop between their respective interpretation of the data, with damage to the credibility of both sides At the same time, a scientific assessment process that is symbolically insulated from the appearance of politics may play a critically important role in certifying that evidence conforms to standards judged acceptable by the scientific community.

Judicial review
Evidence from empirical studies of decision making indicate ... that scientific reviewers ask agencies many of the same questions that courts traditionally have asked pursuant to the "hard look" doctrine: Is the analysis balanced? Does it take account of all the relevant data? Do the conclusions follow rationally from the evidence? Is the analysis clear, coherent and presented in an understandable manner? (Jasanoff, 1990) By virtue of their specialized training and experience, scientific reviewers are likely to be more effective than judges in evaluating agency responses to such questions. There is little evidence that courts, for their part, clearly understand the role and limits of scientific review or have begun to think about the appropriate relationship between review by expert panels and judicial review. However, despite their functional similarities, scientific review and judicial review are not, in the final analysis, equivalent processes. No matter what an expert panel says about an agency's analysis of science, courts have an independent duty to ensure that regulatory decisions comply with the law. Courts should [also] expect to play an assertive role in reviewing cases where agencies and advisory committees disagree in their readings of the scientific record or when there is evidence of impropriety in soliciting scientific advice.

Jasanoff, S. (1995). *Science at the bar: Law, science, and technology in America*. Cambridge, MA: Harvard University Press.

Preface. The goal of the book is to: understand how the legal process mediates among conflicting knowledge claims, divergent underlying values, and competing views of expertise (xiv). Seen up close, legal disputes around scientific facts often appear as sites where society is busily constructing its ideas about what constitutes legitimate knowledge, who is entitled to speak for nature, and how much deference science should command in relation to other modes of knowing (xv). It follows that one cannot fully comprehend the place of science and technology in America w/out closely attending to their deployment in the legal process (xvi). There are two traditions w/r to science in courts (5): (a) Science in policy and (b) Policy for science.

Chapter 1. Courts are a retrospective, case by case approach may seem incompatible w/ the nation's need for national policy (6). ... Demands for better science in the legal process chronically overestimate the power of experts to rationalize moral and political choices about science and technology (7).

Cultures of Legal and Scientific Inquiry - Differences most apparent w/ regard to fact finding (9). Science- concerned primarily w/ getting the facts right. Law - seeks to get facts right, but only as an adjunct to its transcendent objective of settling disputes fairly and efficiently. This leads to a host of secondary contrasts - The law needs closure, it is bounded in time. The law must take a position based on the facts at hand, however premature such a decision may appear in the eyes of science. What the legal fact finder knows is a function of what witnesses chose to relate in response to questions asked by the lawyers. Versus scientist's first hand observation and experimentation.

The rapidly growing roster of technology related disputes in the court challenges any simple characterization of the role courts play in mediating fit between science, technology, and society (16). These cases alert us to subtle connections that exist between conflicts over knowledge, the traditional

preserve of science, and conflicts over responsibility, the classic preserve of the judiciary (16). Many of these cases can be interpreted as struggles over the authority of knowledge (19). Whose knowledge should count as valid science, according to what criteria, and as applied by whom. When should lay understandings of the phenomenon take precedence over expert claims to superior knowledge?

Functions courts serve: (a) Deconstructing expert authority - Litigation is an especially potent force for making transparent the values, biases, and social assumptions embedded in many expert claims about physical and natural phenomena (20) Exposing these underlying subjective preconceptions is as important in a justice system as in getting the facts right Courts can play a useful role also in exposing the "interpretive flexibility" in the meanings that technology has for different social actors. (b) Civic Education (c) Effectiveness - Citizens seeking redress from the courts expect a decision exhibiting certain practical as well as moral attributes

Judicial review has provided inconsistent guidance in interpreting complex environmental statutes and exposed regulatory agencies to new crises of credibility (205) [her conclusion from her review]. Congress's unwillingness to act left courts by default to take lead in constructing new social and political orderings around science and technology. Lawsuits we must conclude are an essential part of the process by which American society comes to grips with the moral, material, and institutional dimensions of technological change

Chapter 10 Myths of Mainstream science. Contrary to mainstream beliefs by many in science and industry, good science is not a commodity that courts can conveniently shop for in some extra-societal marketplace of pure knowledge (207) There's no way for the law to access a domain of facts untouched by values. Historically, sociologically, and politically the suggestion that courts should increase their reliance on value neutral mainstream science is extremely problematic. Notes need for context specific info. in court cases that has limited relevance to scientific discovery and therefore may not be independently reviewed or published. Courts like regulatory agencies conduct the bulk of their scientific inquiries at the frontiers of scientific knowledge where claims are uncertain, contested, and fluid rather than against the backdrop of largely settled mainstream knowledge (210). Instructing courts to take their cues for "idealized stories" of good and bad science can be deeply misleading. Other forms of guidance are needed, more realistically attuned to the indeterminacy of scientific knowledge in the actual contexts of litigation and mindful of the institutional strengths and weaknesses of judicial dispute resolution.

The record of judicial accomplishment
(a) Deconstructing expert authority (211) Cross examination in court focuses on the personal credibility of the witness obsessive concern w/ inconsistencies in the witness's testimony & w/ biases such as ties to economic interests & other common sense ties to credibility. As opposed to science which focuses on methods and assumptions. (b) Civic education - Review by generalist judges symbolizes this nation's continued adherence to the principle that all government actions however arcane or esoteric must be explained in terms that are comprehensible to nonexpert audiences (215) By repeatedly insisting on their prerogatives in this regard, courts have repeatedly affirmed that the ultimate power to guide technology policy is vested not in experts but in the citizenry. Chapter 4 & 7 suggested judicial review helped promote an over dependence on technical rationalization that made policy making look superficially open and participatory but buried values beneath a veneer of objectivity (footnote refers to reference contrasting this w/ European system). Never the less the overall effect of maintaining a dialog bet. experts and people, mediated by the legal process, appears to have been salutary. (c) Effectiveness - Modern technology raises wrenching questions about moral issues (217)
issues which 20th century America seems singularly reluctant to provide collective answers to until multiple possible responses have been articulated in many discrete controversies. There is much to be gained in a pluralistic society addressing value laden technological disputes away from the glare and publicity of national legislation the relatively decentralized, small scale, and ad hoc character of judicial

decision making permits a more leisurely consideration of moral and ethical questions than is generally possible in a legislative arena.

Policy reform: criticism w/ credibility - Separatist schemes Daubert ruling likely to encourage move for judges to think like scientists (219). However her analysis suggests that attempts to reduce all scientific practice to a unitary, reductionist model will only confuse legal thinking and produce more uncertainty. Publishing guidelines of "knowledge" in an area are for judges Problem of obsolescence (220). may lead judges to fail to question the origins and foundations of consensus that a manual purports to represent. Courts should be seen as a "bridging institution" they are places where scientists, lawyers, and lay persons participate in the production of legally relevant knowledge under particular ritualized conventions for establishing credibility and authority. The objectives of criticism are not always best entrusted to litigation. Scientific advisory bodies in particular have demonstrated capacity to synthesize a common knowledge that satisfies norms of scientific, legal, and political accountability. She contrasts EPA nonadversarial Science advisory board with their adversarial Scientific Advisory Panel.

Training judges and informing juries - reference to cross cultural infusions. "Law, like language, is a system in which the elements are mutually interdependent in ways that only a native speaker can fully appreciate." Grafting procedural devices from 1 legal system to another offers only the slimmest hope for success.

One of the greatest strengths of the legal proceedings is precisely the ability to produce localized, context specific epistemological and normative understandings that are not subordinated to inappropriately universal claims and standards (222). Deconstruction of expertise often happens most effectively through repeated encounters between scientists and lawyers with the mix (experts, facts, legal rules) all changing from one disputing context to another. Any changes/reforms then should respect the diversity of problem solving approaches currently represented in the American legal system.

King, P. S. (1995). Applying Daubert to the 'Hard Look' requirement of NEPA: Scientific evidence before the Forest Service in Sierra Club v. Marita. *Wisconsin Environmental Law Journal, 2*, 147-141.

Daubert guidelines were modeled after scientific methodologies (148). These can and should be used to guide evaluations by administrative agencies. King advocates a two step "hard look" doctrine for judges: (1) determine if evidence is scientifically valid and (2) assume that only scientifically valid considered. (Note this refers to evience not the decision - Once the evidence is presented to the decision maker and is evaluated for reliability and relevance, the actual decision can be influenced by other considerations such as politics and economics (150).

Daubert provides the standards for the two step judgment process described above. Looking at Sierra Club v Marita (Seventh Circuit Court), King concludes that the Daubert guidelines were not applied (148).

Science: Most characteristic of scientific thinking is the demand for verification - its habit of skepticism (151). Even established views are subject to rejection. The level of acceptance of an hypothesis in a scientific community is generally proportional to the amount of evidence supporting it (153). The level of general acceptance in the scientific community was the criterion used for years in the Frye test (153). Frye was replaced by Daubert - in addition to acceptance: testability, peer review, and publishing the potential error rate.

Daubert deals with admissibility of scientific testimony at a trial. King argues it should be applied to the validity of scientific evidence before an administrative agency. There are two prongs in Daubert (154) (1) is it scientifically valid (reliable) and (2) can it be applied to facts in this case (relevant).

Lempert, R., & Sanders, J. (1986). Chapter 4: Case logics: Some determinants and consequences. In *An Invitation to Law and Social Science* (pp. 60-88). New York: Longman.

As noted in the conclusion to Chapter 3, adjudications are open in the sense that responsibility is determined only as the answering process proceeds, but the answering process is itself patterned. The difficulties in predicting the outcomes of particular cases do not mean that one cannot explain how cases typically are processed or give the reasons that they are processed in certain ways. In the first section of this chapter we argue that typified analyses of circumstances and normalized meanings tend to be associated so as to form what we call shallow case logics (occur when social/typical meanings satisfy whatever situational demands adjudication imposes) and deep case logics (are likely when the adjudicatory search is for an actor meaning, that is an understanding of the action from the actors point of view). In the second and third sections we examine factors such as the adjudicator's knowledge and objectives and the structure of adjudicative setting that lead adjudicators to adopt shallow or deep case logics. In the last section we note how different case logics produce different meanings of justice.

Madden, E. (2003). Seeing the science for the trees: Employing Daubert standards to assess the adequacy of National Forest management under the National Forest Management Act. *Journal of Environmental Law & Litigation, 18*, 321-364.

Conclusion - Scrutinizing the reliability and relevance of scientific evidence proffered at trial is essential to a fair and just outcome. Assessing the scientific foundation for agency decisions, which require a scientific basis, is no less important. Administrative agencies make decisions every day that affect the health of American citizens and our environment. The Forest Service is no different. The reliability and relevance of the scientific basis for its decisions is of the utmost importance because such decisions affect the water we drink, the air we breathe, and the forests we rely on for recreation, solitude, and economic prosperity. Employing Daubert principles in judicial review is entirely appropriate within the administrative law scheme to ensure reasoned decision-making that is documented and substantiated, as well as to improve agency accountability. Additionally, it is a necessary reform to counterbalance agency bias, increase public trust in the agency, and create consistency in the federal judiciary's approach to NFMA analysis.

Meyer, C. (1999). Science, medicine, and the U. S. common law courts. In C. Meyer (Ed.), *Expert witnessing: Explaining and understanding science* (pp. 1-29). Boca Raton, FL: CRC Press.

Two recent U.S. Supreme Court decisions, Daubert and Joiner, have given federal trial judges power to determine the reliability of scientific opinions and have strengthened their discretion as gatekeepers of scientific evidence. The Federal Judicial Center has provided a reference manual to assist trial judges with the implementation. The stated purpose is clear and laudable - namely, to make it harder for forensic experts to present psuedo-scientific opinions to juries. The unstated purpose is more controversial - namely to make it harder for plaintiffs to win. The Daubert criteria are effective in achieving the latter, but the conflict between the two goals makes it difficult for trial judges to justify their decisions in terms of the first. Accordingly, the application of Daubert has been inconsistent, and the federal appellate judges circuits have been almost evenly split on where the border lies between scientific and nonscientific experts ... This uncertainty threatens an important purpose of law, namely to provide a predictable outcome. It is now more important than ever that judges, experts, and litigators better understand the strengths and limits of their own and each other's fields.

... Two hundred years ago it was still possible for an educated person to gain a satisfactory overview of the cultural world of his own society ... the amount of knowledge and the sophistication of tools have increasingly forced scientists to specialize and choose subfields of science... The scientific world is now

large that despite the intimate international contact among specialist groups, it is no longer feasible for a university professor ... to keep fully abreast in all subfields of his own academic specialty.

Pasko, B. S. (2002). The great experiment that failed? Evaluating the role of a committee of scientists as a tool for managing and protecting our public lands. *Environmental Law, 32*, 509-548.

This article uses the apparent failure of NFMA and its two committees of scientists as a case study to examine the inability of science to effectively manage public lands absent a clearly defined management framework established by policy makers (abstract).

In the end, the courts have not been the focal point in bringing modern science into forest management. Instead, pressure from environmental groups and the general attitude of the public has been the source (536).

The society of American Foresters has expressed the need to distinguish the role of science in policy formation from the role of in decision making (Alexander et al. 1999) (543). Decision making = choice of an alternative from a competing set of alternatives. Policy making = a pattern of action extending over time. Science is a tool of analysis. Science should be used only to form a broad knowledge base. It is not capable of choosing among alternatives that it helps to define. This requires combing scientific and logical reasoning with social values (544).

The initial committee of scientists failed because they played a significant role in developing a set of regulations so complex they were never fully implemented. (544) Perhaps because scientists were taken outside their field of interests and asked to develop policy rather than provide information necessary for sound decision making. The second committee of scientists served in a more appropriate role. Review and analysis of information. They were not involved in making policy. But the Forest Service may have followed the recommendations too closely, following a route that does not coincide with the broader policy expectations of Congress. (545)

Rosenbloom, D. H. (1997). *Public Administration and Law.* New York: M. Dekker.

The Judicial Enterprise - Values Diversity and Individuality: The judiciary's appreciation for diversity and individuality is also ingrained in the case method and adversary proceeding. The judicial approach to decision making requires that an individual be allowed to state his or her own personal case before the courts. Social scientific generalizations may be used, but ultimately judicial decisions turn on the facts presented in individual, idiographic circumstances, sometimes affecting only one or a few people directly. (See, e.g., McCleskey v. Kemp, 1987, for a dramatic example.) In this fashion, great principles of constitutional law and judicial policy making may spring from circumstances involving such seemingly minor incidents as high school pranks. The courts generally rely upon individuals to bring out not only the facts of an incident, but also the legal principles they believe should govern its resolution. Such a method demands a great appreciation of individual worth and the devotion of attention to the diverse claims of individuals. (Rosenbloom, 1997:309)

Stevens, G. (2002). Faulty Science undermines management of natural resources. *Legal Backgrounder, 17*(29), 1.

Each year, hundreds of administrative appeals and scores of lawsuits are filed challenging the ways the US Forest Service and other federal agencies manage or fail to manage our national forests and the wildlife that live there. Typically, this high stakes litigation generates considerable emotion from environmental activists on the one hand, and affected businesses and communities which are often dependent on these resources for their livelihoods, on the other. Nevertheless, all of the parties agree on

one guiding principle - that good science is the necessary foundation for resolving these conflicts. This can create a serous burden for government wildlife scientists, who must strive to put their personal values aside in the interest of producing the best objective science. This increasingly appears to be too much to ask of mere mortals. Courts, therefore, must find effective ways to ensure that wildlife science at the heart of these disputes remains objective and reliable. To do this successfully, courts must steer a careful course between respecting agency discretion and rigorously testing methodological assumptions underlying science. Paper reviews three court cases illustrating the Court's role in this issue.

Media, Politics, and Public Knowledge

Ezrahi, Y. (2004). Science and the political imagination in contemporary democracies. In S. Jasanoff (Ed.), *States of Knowledge: The co-production of science and social order* (pp. 254-273, 304-305). London: Routledge.

Chapter focuses on implications of the decline of the Enlightenment's synthesis of knowledge and politics and the rise of new configurations of knowing and doing politics which are connected with shifts from knowledge to information and from information to outformations as ... means of knowing and guiding politics. (255)

Wisdom - as form of knowing ... characteristically unformalized and even unformalizable. It comes as very contextually rich meanings, links, associations, and references ... As a form of knowing or teaching, wisdom is not ... easily acquired or teachable, or nor accessible through mastery of technical skills.

Knowledge in the scientific sense is perceived as much more systematically organized and formalized - especially due to its logical and mathematical components. ... Whereas wisdom is characteristic of Chinese culture, ... science is particularly Western. ... Knowledge as it emerged in the West relates to values such as clarity, logical rigor, a sharp distinction between truth and error, conflicts of opinions and the urge for their rational resolution. (256) Wisdom by comparison is inclusive of truth and its opposites, irenic rather than polemic, allusive rather than explicit or public ... (Jullien, 2002). ... The production, certification, and communication of scientific knowledge engages a host of methodologies and tools whose ... import has the effect of decontextualizing and depersonalizing claims of knowledge, thus rendering them particularly useful in the production of modern democratic order. ... by contrast to wisdom, scientific knowledge and skills are presumably teachable ... although scientific knowledge, especially in its formal mathematical embodiments may in fact be restricted to specialists, the fact that it can be learned renders it more accessible in principle and therefore ... more democratic. ... The idea of a context free knowledge ... and the tendency to present knowledge in terms of the mirror metaphor as a reflection of the objective properties of the world, made science and scientific authority into invaluable political resources for the construction of the democratic political order. ... But this ... product of an early Enlightenment ... has been gradually replaced by .. the idea [that] context free knowledge is anachronistic, abstract, barely believable and therefore less compelling. ... historical and sociological studies of science ... highlight the fact that [scientific knowledge] has been much more context responsive than many scientists would have us believe.

Information - the transition from knowledge to information ... has been a response to the need to keep knowledge objective or technically valid in, not independent of context. (257) The way to achieve that was to localize objectivity and contexualize technicality in relation to specific goals or tasks. ... By comparison with knowledge it has been usually more detached from the theoretical context in which it was produced, systematically conceptualized, and justified. ... It is knowledge stripped of its theoretical, formal, logical, and mathematical layers and made to fit quick, often "do it yourself" tasks... Matching drugs with symptoms without getting into the deeper structure of disease and the chemical processes which particular drugs induce is, for example, a matter of information. ... As a socio-cultural

configuration, information tends to conceal the interpretive layers and normative commitments underlying its structures and uses. When users discover that being informed is not sufficient to solve a problem ... they may go back to the knowledge base of the information they possess in order to discover why their operations or expectations were thwarted.

Outformations mix the transmission of information with affective, aesthetic, entertaining effects ... aptly called "infotainment" combining norms of entertainment taken from performing arts ... with norms of accurate accounts of reality whose origins are traceable to the culture of science. (259) ... Along the move from knowledge to information, the move from information to outformations often represents the sacrifice of depth and perhaps also accuracy to accessibility. (260) Universal accessibility is apparently more important for the legitimation of contemporary constructions of reality than accuracy, impersonality, "rationality" and other such criteria that relate to the culture of scientific representations. ... Despite the diminished intellectual groundedness of these concepts of the real, their radical inclusiveness or accessibility gives them the required legitimation and fixes them as shared references in the context of public discourse and action.

These changes [from knowledge to information to outformation] raise a host of questions ... the principal query is what are the important shifts in ... in the relations between power, order, and reality? (261)

I would like suggest that we distinguish between what may be called low and high cost realities ... [those] which require heavy investment of resources such as time, money, effort, and skills and those which engage less resources on the part of those who consume those realities. ... Scientific knowledge constructs high cost realities ... [that] tend to be specialized and be demanded by particular professional communities. ... High cost reality is usually a very densely organized system of concepts, facts, clues, rules, interpretive codes, working metaphors, methodological skills, operations, evidence, claims and rhetoric. ... The shift from relying on knowledge to relying on information has often ... translated high cost realities to low cost realities ... furnishing more publicly accessible references to more esoteric claims of knowledge. (262)

Given the extensive commercialization of contemporary mass media, the supply and demand of low cost, low entry threshold realities has become perhaps the most important feature of our culture and politics. (263) ... the Enlightenment conception of politics ... has increasingly appeared to be irrelevant to politics mediated and shaped by what we have called outformations. ... this shift represents a change between alternative cultures of democratic legitimation. .. The replacement of high cost by low cost political reality reflects the diminished propensity of contemporary publics to invest personal or group resources in understanding and shaping politics and the management of public affairs (Putnam 1995; Jasnoff 2001). ... One of the most intriguing questions raised by this state of affairs is why do contemporary publics have such a reduced willingness to invest in understanding and influencing politics? (264)

Just as the scientific enterprise was based on the belief that discrete individual scientists can evolve a coherent and objective body of knowledge; the modern liberal democratic presumption that individuals are the ultimate source of political power and authority does not appear to have foreclosed the possibility that individuals as discrete starting points for the construction of the political order can generate coherent, whole political systems. ... The idea of objective political knowledge has a central element in this new modern model of a polity ... In politics, democratization came to the world with an increasing cultivation of the ideals of a transparent universe of knowledgeable, or at least informed citizens. (265) ... Public knowledge was supposed to be a principal means of disciplining political speech as part of the general attempt to control arbitrary political authority and power (Skinner 1996).

If knowledge is usually perceived as something possessed by scientists and other experts, information and outformation appear more external and independent representations of the world. (266) ... Informed

consent was introduced into medical practice in some countries in the 1970s in order to guide doctors in providing their patients with the medical information relevant to the choice of treatment. The idea was that patients should participate in such choices in order to reflect their personal values and judgment. The point of this change was that health can no longer be regarded by doctors as something definable with reference to a uniform, fixed value order ... informed consent by reducing the power of doctors to influence the choices of medical treatments added legitimacy to the decisions taken. ... While in the case of scientific knowledge, the professional scientific community can still act as guardian of the correct and warranted use of knowledge, in the case of information, the presence of such guardians is diminished, although not altogether eliminated. (267) ... Outformation - while the enhanced accessibility constitutes a democratic gain, the erosion of discernible grounds and methodologies of criticisms constitutes a democratic loss.

Four types of political freedom ... (1) Freedom of knowledgeable and informed citizens from being dependent on the opinions and authority of others (the classical Enlightenment notion) (271) ... (2) freedom to escape the constraints of factual reality .. to live in worlds of fantasy created by poetry, literature, ... (3) Freedom of citizens to collectively imagine into existence a political world of their own making ... (4) the ability to freely switch back and forth between freedom types 1, 2, 3.

Graber, D. (2004). Mediated politics and citizenship in the twenty-first century. *Annual Review of Psychology, 55*, 545-571.

In modern journalism developed out of the necessity to professionalize news gathering, news interpretation, and news distribution tasks. (546) ... When the job became too time consuming and difficult for average people, societies needed specialists who devoted their energies to collecting significant information deemed of interest to their communities and to distribute it cheaply and quickly to interested fellow citizens. ... de Tocqueville's view on the importance of the press (547)... a tool for shared political socialization through which people learn basic values and political orientations to which their society subscribes. ... Democratic governance requires populations that share a sense of national identity and a consensus about major public actions required to protect the collective welfare, collects information about important political events and frames it into news stories that report the salient facts in a context that gives them meaning, mobilizes citizens to take action when needed for the public good, and monitors what government officials are doing and alerts citizens to misbehaviors. ... Social scientists have convincingly demonstrated that people do, indeed, form their impressions about the political world from a succession of stories gleaned from news media (Graber 2001, Kahn & Kenney 2002, Patterson 2002, Zaller 2003). ... The "priming" phenomenon is an especially interesting media effect because it confirms that people do, indeed, absorb information from news stories and use it to guide subsequent thinking. ... It also confirms the well-documented human preference for "satisficing" rather than "optimizing" ... If schema that have been recently primed by news stories allow them to form opinions, they prefer to probe no further (Anderson 1983).

Turning briefly to the press as a tool for shared political socialization: It has indeed served that function in the past. But the process has become increasingly difficult because the U.S. population now represents a much broader array of ethnic and religious traditions and spans a much wider range of socioeconomic and educational experiences. The cultural melting pot has given way to multiculturalism. ... The fear is that people will find it increasingly difficult to agree on common political agendas and that norms of tolerance that are so crucial in democracies may weaken (Dahlgren 2001, Sunstein 2001). Rather than participating in nationwide dialog, people may abandon the previously shared public sphere and retreat into a multitude of communication ghettoes (Bennett 1998, Entman & Herbst 2001, Swanson 1997).

There is a huge gap between democratic theorists' expectation and the reality of how much political knowledge the media will transmit and what citizens can and will learn (Bartels 1993, Iyengar & Simon

2000). Journalists ... do [not] see political education of the public as their primary role (Weaver&Wilhoit 1996) ... The vast majority of average citizens, contrary to theorists' hopes, survey political news haphazardly, spending less than an hour daily on it.

Sociologist Michael Schudson (1998) has pointed that out in his studies of the changing nature of citizenship. Traced successive stages of citizenship moving from the ideal of the "deferential citizen" in the eighteenth century to the model of the "partisan citizen" prior to World War I and the ideal of the "informed citizen," highly knowledgeable about the intricacies of politics, thereafter. That model held sway until the 1960s when it became increasingly difficult, even for the well educated, to keep abreast of public affairs. According to Schudson's analysis, the informed citizen model now has given way to the more realistic "monitorial citizen" model. Monitorial citizen model ... Reliance on information shortcuts yields acceptable results (Elkin & Soltan 1999; Mondak 1994; Norris 2000a,b; Popkin & Dimock 1999). ... Unlike the fully informed citizens of the prior period, monitorial citizens need not stay fully informed about political developments at all times. They only need to survey the political scene carefully enough to detect major political threats to themselves or their communities. Some wrestled with the details as required by the informed citizen model to determine which ballot initiative matched their interests best. Others simply ascertained who favored and who opposed each initiative and then sided with their presumed friends. Both sets of voters managed to match their vote to their own welfare with only a slight disadvantage for the group using heuristics (Lupia 1994). ... While their performance is far from the ideal models of the past, it is reasonably adequate for fulfilling major citizen responsibilities such as discussing politics intelligently and voting for candidates and ballot propositions. The monitorial citizen model stresses that the need for citizen alertness is cyclical-greater in times of crisis and less at other times. Citizen behavior conforms to such cycles.

Following in Schudson's (1998) footsteps, I have pleaded that monitorial citizenship is a realistic, politically sound concept. Most scholars in the decision sciences, psychology, economics, and even political science accept the idea that the human capacity for absorbing information is limited (Simon 1995).

Media may play their most important role in supplying the news to the attentive segment of the public that, in turn, routinely relays political information to less-interested fellow citizens. The media's role in informing monitorial citizens may thus be a two-step process. Attentive publics are aroused first by news stories that provide adequate data. Their concerns and recommendations are then transmitted through print and broadcast news stories to less attentive citizens, making it easy for these citizens to form and express sound opinions. Complex modern societies require intermediaries between citizens and elected and appointed public officials. Relatively small groups of attentive citizens have always served that role along with political parties and interest groups. These proxies relieve the majority of citizens of the burden of continuously monitoring public problems and pondering solutions.

Graber, D. (2003). The media and democracy: Beyond myths and stereotypes. *Annual Review of Politcal Science, 6*, 139-160.

This essay's point of departure is the hallowed belief that democracy requires active citizens and news media that supply them with information they need to participate effectively in politics. The main features of this model of a functioning democracy, including the underlying assumptions, are tested and found wanting. Neither citizens nor media are capable of performing the roles expected of them. The appropriateness of these roles for life in modern societies is also open to question, as are the many myths and stereotypes that obscure the interface between media and democracy. The fact that democracy can persist despite citizens and media that fall short of the expected performance suggests that political culture may be more important than citizen wisdom and media excellence. Rallies in civic activism during crises may also be a major factor in the durability of democratic governance in the United States.

Harding, S. (2000). Should philosophies of science encode democratic ideals. In K. D. L. (Ed.), *Science, Technology, and Democracy* (pp. 121-137). Albany, NY: State University of New York.

The technical, cognitive elements of scientific practices and the info these produce always represent social and political priorities, meanings, and ideals as well as more or less accurate pictures of nature's order. ... One can never be sure the sciences have arrived at absolutely true claims for two reasons: (1) present claims must be held open to revision in case of the appearance of further empirical evidence and (2) they must be held open to the need for fruitful conceptual shifts. The above 2 factors are what are supposed to distinguish empirical science from dogmatic positions... Most of the observations by medieval astronomers are still facts w/in astronomy today, yet the hypothesized relations between these facts and the means such facts have in the modern world are vastly different from the relations and meanings attributed them in the medieval world. ... (124) ... There are many respects in which philosophies of science could encode democratic ideals, this chapter focuses on just one - the idea that the "universality ideal" is scientifically and political dysfunctional ... it devalues cognitive diversity which is now and always has been an important resource for the growth of knowledge. (125)

Some aspects of the philosophy of science already encode democratic ideals (125). For example, science insists that the social status of the observer should not provide a standard for evaluating the adequacy of scientific observations, results must be replicable by any individual or groups. This requires that research must be public science belongs to humanity and may not legitimately be shielded from public view. The required publicity of results of research is an important democratic ideal that is part of scientific method. (126)

But what is the guiding general democratic principle? One possibility - those who bare the consequences of decisions should have proportionate shares in making them (127)

Modern science is plural - incompatible ontologies, methods, and models of nature/research processes (130) No longer is it reasonable to most philosophers and scientists to try and explain the phenomena of interest to biology using the concept of physics. Should think of harmony rather than unity - sharings, borrowings, communications - borrowings have been a continual source of new insights in every field... One could say attempts to universalize a belief is simply the attempt to see in what contexts it can gather empirical evidence and prove useful. (136) Harding argues for prioritizing the development of significantly different knowledge systems rather than of one perfect system. (137)

Lange, J. I. (1993). The logic of information campaigns: conflict over old growth and the spotted owl. *Communication Monographs, 60*, 239-257.

This descriptive, qualitative case study examines a resource conflict in which advocates' and counter advocates' rhetorical and communicative strategies mirror and match one another as disputants engage in a synchronous, spiral like logic of interaction. The analysis reveals how disputants; interactive logic - a mirroring of each others' strategies is achieved with little to no direct communication between parties. They learn of each others tactics through the mass media as they pursue duplicate or antithetical rhetorical strategies w/ various audiences. Poole (1984) remarked how negative spirals can become self perpetuating while Hocker and Wilmot (1991) argued that communication actually accelerates misunderstanding when contextualized in destructive escalatory spirals (242)

Four parallel vilification strategies identified by Vanderford (1989) were employed: (1) frame/reframe, (2) selective use of information, (3) vilify/ennoble, and (4) simplify and dramaticize (Zarefsky (1992:412) argues that American audiences find complexity unbearable and that we simplify what can not be avoided (250)

By examining the two competing information campaigns of this conflict, we find a logic of interaction between two groups who rarely communicate with each other. At least a large part of what the opposition is doing and thinking in this case is plotting how to respond to the other group. Bennett contended that the nature of contemporary political dramas threaten the vitality of democracy (1992:403). That the timber and environmental campaigns either match or mirror the other advances an additional explanation for understanding why it is so difficult for campaigns to rise above the current state of affairs: their co-created systems preclude it (254). Parties are compelled to respond to each other in ways determined by the system they constitute (Pearce 1989) the system is the best explanation of itself - A specific communicative act by 1 interlocutor practically forces a predetermined response by the other. Parties become locked into a systemic, self-reinforcing patterned and repetitive practice. The consequences of not mirroring or matching one's antagonist are untenable. If parties who create American political information campaigns are somehow locked into an inherently flawed system from which they retreat only at their peril, we have indeed reached the crisis in political communication about which Zarefsky (1992) warns.

Philosophy of Science

Addis, M., & Podesta, S. (2005). Long life to marketing research: A postmodern view. *European Journal of Marketing, 39*(3/4), 386-412.

In this paper, we try to interpret the epistemology of marketing, a specific part of management theory, by conducting an analysis of the literature as it has developed so far, and constantly creating links between the level of philosophical elaboration and that of marketing research. ... (387) The analysis in the next pages will highlight how ... marketing has partly diverted researchers' attention from the theory, and focused it mainly on the method: a distorted mechanism was created which guaranteed the scientific nature of the discipline by using scientific methods considered universal and immutable. The focus on the method, derived from the need to make marketing a discipline with an academic status, has created an increasingly marked distinction between the marketing literature aimed at management, and that aimed at the academic community. While the former tends to stress the managerial implications of the contribution (they are, therefore, operative and tangible implications), the latter mainly insists on the adoption of a scientific method, which, in very complex contexts, is often coupled with excessive specialization, although supported by sophisticated modeling.

This paper analyses the philosophical roots of postmodernism, in order to understand its impact on postmodern marketing better (abstract). It also focuses on the impact of postmodernism on marketing research, and proposes a new approach. This paper then explores the features of the experiential research in marketing, and its effect on the processes of generating knowledge.

The vicious circle in which marketing has fallen into seems to lead it towards an involution with no a way out. (403) If it is true, as Brown (1997) states, that with postmodernism we entered the era of anti-science, the future of marketing is obscure and difficult to see. Only if marketing researchers acquire responsible awareness will be possible to have a brave re-directing of the discipline. Researchers who tried to deal with postmodern marketing (among whom Sherry, 1991; Brown, 1993, 1994, 1995, 1997, 1998, 1999; Holbrook, 1993; Thompson, 1993; Bouchet, 1994; Elliott, 1994; Firat and Venkatesh, 1995; Uusitalo, 1998; Cova and Cova, 2001) concluded their works by inviting marketing researchers to consider the limits of the modern marketing philosophy, thus joining the "marketing-is-not-working manifesto" (Brown, 2002).

Daston, L. (1992). Objectivity and the escape from perspective. Social Studies of Science, 22, 597-618.

Scientific objectivity is neither monolithic nor immutable: our current usage is compounded of several meanings - metaphysical, methodological and moral - and each meaning has a distinct history, as well as a history of fusion within what now counts as a single concept of 'objectivity'. The rise of aperspectival history in the 19th century science is one strand of this plaited history of objectivity, as embodied in scientific ideals and practices. It is conceptually and historically distinct from the ontological aspect of objectivity that pursues the ultimate structure of reality, and from the mechanical aspect of objectivity that forbids interpretation in reporting and picturing scientific results. Whereas ontological objectivity is about the fit between theory and the world, and mechanical objectivity is about suppressing the universal human propensity to judge and aestheticize, aperspectival objectivity is about eliminating individual (or occasionally group) idiosyncrasies. It emerged first in the moral and aesthetic philosophy of the late 18th century and spread to the natural sciences only in the mid 19th century, as a result of reorganization of scientific life that multiplied professional contacts at every level, from the international commission to the well-staffed laboratory. (Abstract)

Our usage of the work 'objectivity' ... is hopelessly but revealingly confused. It refers at once to methaphysics, to methods, and to morals. We slide effortlessly from statements about the 'objective truth' of a scientific claim, to those about the 'objective procedures' that guarantee a finding, to those about the 'objective manner' that qualifies a researcher. (597). The purpose of this paper is to contribute to an understanding of part of the history of the meaning of objectivity. Specifically she traces the rise and ascendancy of the meaning of 'aperspectival objectivity' in 19th century science since it dominates current usage. (599)

Aperspectival objectivity is both conceptually and historically distinct from the ontological objectivity that pursues the ultimate structure of reality and from the mechanical aspect of objectivity that forbids judgment and interpretation of scientific results. (599) Where as ontological objectivity is about the world, and mechanical objectivity is about suppressing the universal human propensity to judge and to aestheticize, aperspectival objectivity is about eliminating individual (or in some cases group) idiosyncrasies.

In the 19th century aperspectival objectivity became the creed of scientist ... an ideal that corresponded to the practice of ... constant, impersonal communication. (609) As Theodore Porter (1992) has argued, certain forms of quantification have come to be allied with objectivity not because they necessarily mirror reality more accurately, but because they serve the ideal of communicability, especially across barriers of distance and distrust. ... Subjectivity became synonymous with the individual and solitude; objectivity with the collective and conviviality.

I have left many questions unanswered, chief among them how aperspectival objectivity came to be fused with other meanings of objectivity into a single, if conglomerate concept. (613) Why, for example, should public knowledge - observations most easily communicated to and replicated by as many people as possible - lay metaphysical claims to being the closest approximation of the real?

Dupre, J. (2002). The lure of the simplistic. *Philosophy of Science, 69*, s284-s293.

Simplicity has often been suggested as a virtue of scientific theories or scientific explanation, and perhaps it is. But this is also a domain in which the simple all too often merges into the simplistic. Or so I shall claim. This paper attacks the perennial philosophical and scientific quest for a simple and unified vision of the world. Without denying the attraction of this vision, I argue that such a goal often seriously distorts our understanding of complex phenomena. The argument is illustrated with reference to simplistic attempts to provide extremely general views of biology, and especially of human nature, through the theory of evolution. Although that theory is a fundamental ingredient of our scientific world view, it

provides only one of a number of perspectives that are required for an understanding of biology in general, and human behaviour in particular. The argument is connected to the replacement of views of science in terms of universal laws with views that emphasize ranges of models more locally suited to specific phenomena.

Simplifiers or unifiers will hope to show that one fundamental theory will provide the key to a whole domain such as that of Life on Earth, or even just Life. Anti-simplisticators will insist that even a theory as profound as this one will illuminate only certain aspects of a domain of this complexity. ... the majority of philosophers (and perhaps biologists) to see evolution not as providing us with some kind of universal law in the style of Newtonian mechanics, but rather as providing a large and open-ended family of models, a tool-kit with which we can hope to get some understanding of the processes involved in particular cases. But this move is fatal for unifying aspirations. A universal law carries with it, so to speak, the instructions for applications to new cases. ... Models aren't like that. A model is more or less isomorphic to the situation it is supposed to model. ... Models are only transportable to new situations to the extent that the same factors are important and no new ones are important. This is a very restrictive condition, and one that needs to be specifically justified in any particular case. And this is of course why we aim to assemble a toolkit of models. The more diverse our tool-kit, the better chance we have of finding something that may serve the case in hand.

Let me conclude with a more ambitious proposal. Human behavior is in one sense biological, but it is also fundamentally social. It is culturally diverse. On occasion it is economic, religious, domestic, competitive or cooperative, and so on. It is, in short, exceedingly complex. Any grand unifying theory of human nature can be confidently predicted to distort many or most of these complexities. It seems to me that here we should not even aspire to approach the topic with a unified tool-kit. Indeed, the more diverse and varied the contents of our tool-kit, the better chance we have of coming to grips with the really interesting problems about human behavior. Just as simplicity or simplisticity is naturally connected with unity, so complexity cries out for plurality. The only route to a deeper understanding of ourselves is through radical epistemological pluralism.

Flyvbjerg, B. (2001). *Making Social Science Matter: Why Social Inquiry Fails and How it Can Succeed Again*. Cambridge, UK: Cambridge University Press.

Fuchs, S. (2000). A social theory of objectivity. In U. Segerstrale (Ed.), *Beyond the Science Wars: The Missing Discourse about Science and Society* (pp. 155-183). Albany, NY: State University of New York Press.

Philosophers generally agree that objectivity is the distinctive mark of scientific knowledge; disagreements exist over how it is possible, how it can be secured, and for exactly what sorts of propositions objectivity can be achieved. (156) ... I deal with objectivity as a special kind of social and communicative "medium" that separates science from other modes of communicating. ... As both conduct and knowledge objectivity rises above partial perspectives and idiosyncratic standpoints. ... The knower or observer gradually disappears.

This asperspectival objectivity can be achieved, it is said, by letting the world select its own representations. (157) ... Taking this view requires special efforts. ... Epistemology has suggested various ways of doing this work, all of which seem to have failed, at least in their strongest foundationalist and absolutist aspirations. ... the antifoundationalist and antirepresentationalist consensus in science studies points out that science is "just" a contingent and historical culture ... Two conclusions are drawn from the constructivist critique of objectivity ... objectivity is rhetoric ... objectivity is power. (158)

The argument for objectivity as rhetoric has some merits, but overstates the case. (159) ... the rhetoricist argument gives almost magical powers to the Word, the Text, and Discourse, while ignoring the organizational and material contexts of scientific work. ... That scientists use rhetoric to persuade others is true but trivial ... the more interesting and less obvious question ... is when such rhetorically couched claims to objectivity and disinterestedness are likely to be believed. ... Credible claims to objectivity are supported by much more than just rhetoric ... Discursive and textual practices are only one of the resources available for today's science to make forceful claims to authoritative and objectivity knowledge. ... Rhetoric is at best a necessary, not sufficient condition for objectivity.

The critical theory of objectivity holds that objectivity is a weapon to exclude marginal voices from dominant ... narratives of Western rationalism and empiricism, spearheaded by science ... (160) Some radical critics suggest that objectivity is beyond repair and should be replaced by "stand point epistemologies". (161) These link ways and modes of knowing to interested social locations and positions. ... Standpoint epistemologies seek to make the conditions and circumstances of knowledge distributions visible and accountable. ... Standpoint epistemologies are correct to remind us that all knowledge is part of the world, produced by groups placed in social space and historical time. ... [Regarding tendency to criticize positivism] by now there is widespread agreement that philosophy of science, especially positivism, is a very poor guide to actual science. (163) ... Observers of research fronts report that work looks surprisingly "interpretive" (164) ... Innovative science is too uncertain and controversial to have a firm and relentless grip on its victimized objects (164) ... Ironically, the critique of objectivity as totalizing power is totalizing itself. ... There is simply too much variation in the ways of doing science for all of it to follow some unitary instrumental interest in control. ... In fact, the critics of objectivity repeat the main error of epistemology - there is not one method or logic to science, but many methods and logics, tailored to particular specialties, their means of production, and their social structures.

I believe we need to rescue objectivity from both orthodox philosophy and standpoint epistemologies. (166) ... There seems to be widespread agreement now that objectivity is not, and cannot, be universal and foundational, somehow above and beyond society and time. (167) ... I build on Luhmann's theory of social systems ... (169) Objectivty as a medium. As a medium objectivity is not correspondence to realism, not a set of rules or methods, and not an extra-worldly view from nowhere. Rather it is a form of communication.

Communication becomes improbable to the extent that individual minds with unique and special inner experiences cannot be expected to share mental states through mutual empathy and introspection. (170) Communication becomes improbable when writing extends possible audiences so much that there are no longer any guarantees that a communication will actually reach someone. ... Objectivity organizes communication on a more abstract level than theories... (171) A medium only decides what kinds of communications can take place in a given area and what sorts of experiences count as information. ... Truth and objectivity ... do not refer to knowledge that corresponds to some external reality. ... They do not indicate states of the world, but states of a social system, science. ... The theory of media and codes is antirealist and constructivist. It dismisses traditional objectivity and does not return to a precritical stage in epistemology.

The important assumption is that ... decisions are not based on ... power but on arguments and evidence (172). These can usually be ranked according to more or less shared criteria of good versus bad evidence, convincing versus unconvincing arguments, and so on. There can, of course, be disagreements about what counts as good and bad evidence. ... If decisions about truth are changed later, and they always will be, then this is interpreted to mean that learning has occurred. (173) ... And if some finding has indeed been caused by some interest, this is cause for suspicion.

Trust can coalesce around a person, as in love, around social status, as in early modern gentleman science, or around procedures as in modern science. (174) ... This trust in procedural objectivity explains the continuing significance of method in science ... In contrast to fraud, global failures of trust in science are very rare (175) ... In these serious crises, science turns into ideology - what Kuhn calls incommensurability. ... The less they communicate, the more the opponents really do start living in different social and intellectual worlds. (176)

Conclusion - argues for objectivity as a mode of trusting communication. (177)

Giorgi, A. (1997). The theory, practice, and evaluation of the phenomenological method as a qualitative research procedures. *Journal of Phenomenological Psychology, 28*(2), 235-260.

Science is a cultural institution dedicated to the project of gaining the most valid possible knowledge of the phenomena of the world. The qualification "most valid" is what distinguishes science from other forms of knowledge. Not all forms of knowledge qualify as scientific knowledge. In order to be scientific, knowledge must be (1) systematic, (2) methodical, (3) general, and (4) critical.

Systematic - to say that knowledge is systematic means that one expects different segments of knowledge to be related to each other, to be regulated by laws, concepts, or meanings. It means that one expects knowledge to be patterned and ordered rather than chaotic or random.

Methodological To say that knowledge is methodical means that it is gained through a method that is accessible to a community of scholars.

General - To say that results are general means that the knowledge has applications beyond the situation in which it was obtained. While universalization is the highest form of generalization, it is not demanded of all inquiry. This is especially true of the human sciences, where contexts are important and tend to relativize findings.

Critical - Finally, to say that knowledge is critically evaluated means that no outcomes are merely accepted. Rather, they are first challenged by systematic procedures of a given investigator and then published, so that relevant, qualified members of the research community can also submit scientific results to critical scrutiny.

Goodwin, J., & Horowitz, R. (2002). Symposium on methodology in qualitative sociology. Qualitative Sociology, 25(1), 33-47.

Some social scientists view qualitative sociology, in no uncertain terms, as methodologically and empirically "soft" and highly subjective, if not completely solipsistic-a characterization that a few qualitative researchers have ironically embraced. At best, according to certain critics, qualitative sociology might generate provisional hypotheses that more rigorous social scientists can then go forth to test and revise, but it cannot itself glean much solid understanding of the social world. We believe that this view of qualitative sociology is badly mistaken. At its best, qualitative sociology can be very rigorous and "scientific" indeed. Qualitative sociology, in short, has some very important things to say about the world beyond the researcher.

King, Keohane, and Verba believe that qualitative social scientists need to pursue their research in a more rigorous and scientific manner, which basically means, for them, adhering as much as possible to the standards of quantitative research. (Significantly, they do not ask whether quantitative work might be improved by emulating certain features of qualitative research.) King, Keohane, and Verba suggest that quantitative and qualitative research share "the same logic of inference" (1994, p. 3), and they elaborate a

number of rules for rigorous, scientific qualitative (and quantitative) research (see Munck 1998). Certainly, judged by King, Keohane, and Verba's rules, much qualitative sociology would be found wanting in various ways, perhaps severely. ... For example qualitative research is said to suffer from an alleged "small-N problem," failing to examine a sufficient number of cases for building solid generalizations or good theory. Participant-observers, moreover, are often charged with inducing (or even provoking) much of their data, which means that such data may not be replicable (violating rule 10). More generally, participant-observers have been accused of lacking objectivity or critical distance from the groups or institutions in which they insert themselves.

Of course, just what constitutes qualitative sociology and "its" methodology is notoriously difficult to say. Generally, then, qualitative sociologists-whether street-corner ethnographers or comparative historical analysts-attempt to remain as close as possible to the actual phenomena that they are trying to understand. They believe that their cases, whatever they may be, have to be understood contextually or holistically, and often with attention to temporal ordering.

Can anything be done to make qualitative sociology more "scientific"? And should we try? In fact, as the essays in this symposium demonstrate, good qualitative work is not only empirically rich, but is often more methodologically rigorous than might appear. ... Qualitative methods and research have undoubtedly become much more transparent in the last twenty-five years. ... There has also been a trend in recent years toward greater methodological self-awareness among historical and comparative historical sociologists. ... In the early 1990s, furthermore, the very concept of the "case" as well as the uses and possibilities of single-case studies were critically interrogated (Ragin and Becker 1992; Feagin, Orum, and Sjoberg 1991; Amenta 1991). Again, the result has been greater self-awareness about which "units of analysis" are appropriate and fruitful to examine and compare.

The issue of generalizability is critical to qualitative sociology and to sociology generally. The key trade-off that all social scientists (not just qualitative sociologists) need to weigh has always been and will remain one between depth ("thick," contextualized data) and breadth (large samples of cases). The strength of qualitative research has been to create a deeper and richer picture of what is going on in particular settings, although it has also been able to employ comparisons among a relatively small number of cases to great effect. ... Qualitative analysts will always be challenged, however, to engage and interest people who do not happen to share an interest in the particular case or cases that they write about. We are also challenged to contribute to general explanations of the particular class of phenomena of which our cases form a part. ... Qualitative research has but a few formulas about howto make this connection between the specific and the general. This does not mean, however, that there are no methodological standards available to qualitative researchers or that qualitative studies lack rigor.

Grantham, T. A. (2004). Conceptualizing the (dis)unity of science. *Philosophy of Science, 71*, 133-155.

As philosophers of science, it is important to understand the pursuit of unity: (134) What kinds of unification (integration) are sought? What is the epistemic and/or metaphysical significance of unification in science? In the history of the philosophy of science, many different forms of unity have been discussed, including the unity of language, unity of method, and the unity of theories. Nonetheless, many contemporary discussions focus on what I call the "unity as reduction" (UAR) model. UAR makes two claims: scientific unity is a relationship between theories, and theories are unified when a ... specialized theory is reduced to a ... more general theory. This paper argues that conceptualizing unity as "interconnection" (rather than reduction) provides a more fruitful and versatile framework for the philosophical study of scientific unification. According to this alternative conception, fields are unified to the extent that they are densely connected.

Darden and Maull's (1977) nonreductionist theory of unity. Definition of field - An area of science consisting of the following elements: a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory factors and goals providing expectations as to how the problem is to be solved, techniques and methods, and, sometimes, but not always, concepts, laws and theories which are related to the problem and which attempt to realize the explanatory goals (Darden and Maull 1977). Whereas disciplines are generally understood to have sociological dimensions (e.g., Bechtel 1986), Darden and Maull define and individuate fields on the basis of their conceptual structure. Furthermore, fields are typically much smaller than disciplines. An interfield theory postulates a connection between the entities or processes which are studied by distinct fields. Darden and Maull argue that interfield theories contribute to the unification of the biological sciences without effecting a reduction. Biologists did not attempt to reduce genetics to cytology (or vice-versa). It reduced inter-field conflict and facilitated the flow of ideas between fields-but it did not lead to an intertheoretic reduction.

Grantham's model of unity - 2 theories become more unified as they become increasingly interdependent. Theories can be connected in a variety of ways. Scientists can come to understand how the ontologies, concepts, explanations, generalizations, data, or methods of two theories are related. Even when reduction fails, the concepts and ontologies of the theories may be closely connected and the two theories can be strongly interdependent with regard to their heuristics, methods of confirmation, and explanations conceptualizing unity as reduction generates a problem: whenever reduction is not (in principle) possible, we must embrace the "disunity" of science Most philosophical accounts of the unity of science ... focus on the unification of theories. By contrast I emphasize the unification of fields. Attending to interfiled relations has an important advantage. Because fields contain both theoretical and non-theoretical components (e.g., heuristics and methods), the language of fields provides a richer vocabulary for articulating a wider variety of connections.

Haack, S. (2003). *Defending science - within reason.* Amherst, NY: Prometheus Books.

Science is not sacred: like all human enterprises, it is thoroughly fallible, imperfect, uneven in its achievements, often fumbling, sometimes corrupt, and of course incomplete. (19) Neither, however, is it a confidence trick: the natural sciences, at any rate, have surely been among the most successful of human enterprises. ... Prhaps it is no wonder that many scientists came to regard philosophy of science as at best irrelevant (21) ... in 1987, provoked by Alan Chalmers' observation at the beginning of his popular introduction to philosophy of science, that "[w]e start off confused and end up confused at a higher level" physicists Theocharis and Psimopoulos published an impassioned critique of "betrayers of the truth": Popper, Kuhn, Lakatos, and Feyerabend, "the worst enemy of science." And as the influence of the New Cynicism grew, more scientists moved to defend the honor of their enterprise ...

Haack articulates a Critical Common-Sensist Account (22). ... Though there are elements of truth in both the Old Deferentialism and the New Cynicism, a crude split-the-difference approach won't do; for the truth, as Oscar Wilde so nicely put it, "is seldom pure and never simple." ... The core standards of good evidence and well-conducted inquiry are not internal to the sciences, but common to empirical inquiry of every kind. (23)... respect for evidence, care in weighing it, and persistence in seeking it out ... are the standards by which we judge all inquirers, detectives, historians, investigative journalists, etc. ... In short, the sciences are not epistemologically privileged. They are, however, epistemologically distinguished; the natural sciences at least, fallible and imperfect as they are, have succeeded remarkably well by the core epistemological standards of all serious inquiry. But distinction, unlike privilege, has to be earned; and the natural sciences have earned, not our uncritical deference, but our tempered respect.

Scientific inquiry is continuous with the most ordinary of everyday empirical inquiry. There is no mode of inference, no "scientific method", exclusive to the sciences and guaranteed to produce true, probably true, more nearly true, or more empirically adequate results. (24) ... As far as it is a method, the scientific

method is what historians or detectives or investigative journalists or the rest of us do when we really want to find something out: make an informed conjecture about possible explanations of a puzzling phenomenon, check how it stands up to the best evidence we can get, and the use our judgment whether to accept it , more or less tentatively, or modify, refine, or replace it. Inquiry is difficult and demanding, and we very often go wrong. ... The remarkable success of the natural sciences are due, not to a uniquely rational scientific method, but to the vast range of "helps" to inquiry devised by generations of scientists to overcome natural human limitations. (25) Instruments of observation extend sensory reach; models and metaphors stretch imaginative powers; linguistic and conceptual innovations enable a vocabulary that better represents real kinds of thing and event' techniques of mathematical and statistical modeling enable complex reasoning; the cooperative and competitive engagement of many people in a great mesh of sub-communities within and across generations permits a division of labor and pooling of evidence...

There can no more be rules for when a theory should be accepted and when rejected than there could be rules for when to ink in a crossword entry and when to rub it out; "the" best procedure is for different scientist, some bolder, some more cautious, to proceed differently.

Haack, S. (2001). An epistemologist in the bramble-bush: At the Supreme Court with Mr. Joiner. *Journal of Health Politics, Policy, and Law, 26*, 217-224.

The ... interaction of science and the law raises some very tricky problems. And to judge by how often, in that long and tortuous history, explicit or implicit assumptions about the nature of scientific knowledge and the character of scientific inquiry are crucial, those problems are in part epistemological. ... Almost a century ago, Learned Hand argued that the role of the expert witness ... is anomalous, for if each party presents its own expert witness(es), the jury must decide "between two statements each founded upon an experience foreign in kind to their own"-when "it is just because they are incompetent for such a task that the expert is necessary at all" (Hand 1901: 54). ... sometimes the scientific determinations that judges or juries are asked to make may be so subtle and sophisticated, so manifold and tangled, that even those competent in the relevant area of science may legitimately disagree-or may agree that there is too little evidence, that they just don't know.

Legal efforts to winnow decent scientific evidence from the chaff, I shall argue, have often been based on false assumptions about science and how it works. It doesn't follow, unfortunately, that if we had a better understanding of science, all problems could be easily resolved. A better understanding of scientific evidence and inquiry will reveal why it has proven so difficult to find a legal form of words that will ensure that only decent scientific evidence is admitted, or a simple way to delegate some of the responsibility to scientists themselves; but rather than suggesting any easy solutions it accentuates the need to think hard and carefully about what goals we should be trying to achieve, and what kinds of imperfection in achieving them we are more willing, and what we are less willing, to tolerate.

Nor is there any "scientific method" guaranteeing that, at each step, science adds a new truth, eliminates a falsehood, gets closer to the truth, or becomes more empirically adequate. Scientific inquiry is fallible, its progress ragged and uneven. At some times and in some areas, it may stagnate or even regress; and where there is progress, it may be of any of these kinds, or it may be a matter of devising a better instrument, a better computing technique, a better vocabulary, etc.

As human cognitive enterprises go, natural-scientific inquiry has been remarkably successful. But this is not because it relies on a uniquely rational method unavailable to other inquirers; no, scientific inquiry is like other kinds of empirical inquiry-only more so. As Percy Bridgman once put it, "the scientific method, so far as it is a method, is doing one's damnedest with one's mind, no holds barred" (Bridgman 1955: 535).

Scientific inquiry is "more so" in part because of the many and various helps scientists have devised to extend limited human intellectual and sensory powers and to sustain our fragile commitment to finding out: models, metaphors, and analogies to aid the imagination; instruments to aid the senses; elaborate experimental set-ups to aid in testing and checking by flushing out needed evidence; mathematical, statistical, and computing techniques to aid our powers of reasoning; and a tradition of institutionalized mutual disclosure and scrutiny that, at its best, enables the pooling of evidence and helps keep most scientists, most of the time, reasonably honest.

Hanna, J. F. (2004). The scope and limits of scientific objectivity. *Philosophy of Science, 71*, 339-361.

The aim of this paper is twofold: first to sketch a framework for classifying a wide range of conceptions of scientific objectivity and second to present and defend a conception of scientific objectivity that fills a neglected niche in the resulting hierarchy of viewpoints. Roughly speaking, the proposed ideal of scientific objectivity is effectiveness in the informal but technical sense of an effective method. Science progresses when "higher levels of communicative discourse" are reached by transforming subjective judgments regarding the generation and reduction of data or the testing of theories into objective decision procedures that are automatic or mechanical. (abstract)

In her forceful critique of the dominant conceptions of 'objective' and 'objectivity' in contemporary philosophy, Elisabeth Lloyd (1995, 353) emphasizes this dual aspect or "hybrid" character of scientific objectivity. (340) She notes that sometimes: objective means detached, disinterested, unbiased, impersonal, . . . not having a point of view. . . ; objective means public, publicly available, observable, or accessible (at least in principle); objective means existing independently or separately from us; objective means really existing, Really Real, the way things really are. The first two conceptions identified by Lloyd are internal or methodological, having to do with the rational methods of science; the latter two conceptions are external or representational, having to do with the rational goals of science.

Lloyd has identified a ...disturbing tension in contemporary theories of science. How can one coherently reject the claim that science is "aimed at" an independently existing, objective reality, while maintaining that scientific methods can (and ought to) be neutral, non-ideological, and unbiased The aim of this paper is twofold: (341) to sketch a framework for classifying a wide range of conceptions of scientific objectivity and to present and defend a conception of scientific objectivity that ... suggests at least a partial resolution of the disturbing tension that Lloyd has so convincingly portrayed. Roughly speaking, the proposed ideal of scientific objectivity is effectiveness in the informal but technical sense of an effective method. Science makes objective progress when decision procedures requiring subjective human input are replaced by decision procedures that are automatic or mechanical.

Example: No doubt, there was a period in human history when the simple activity of counting was a discursive activity rather than a straightforward matter of habit. Eventually, as counting became just a matter of techne it was possible for human culture to tackle more complex aspects of arithmetic, such as addition and multiplication. The point - every discipline presupposes a foundation of "unconscious habit" and the more complex and penetrating the discipline the more broadly and deeply entrenched must be the foundation of habits (techne) that support activity at the frontier.

Science makes progress through the replacement of subjective protocol judgments by objective artifactual data. In a similar fashion, methodological progress in the articulation, application and testing of scientific theories results when decision processes requiring subjective human judgment are superseded by decision processes utilizing objective automated computation.
The artifacts-empirical, computational and technological-that result from the development, testing and application of scientific theories are accessible to "public inspection," and in a significant sense they are

"Really Real" and "exist independently or separately from us." However, like all artifacts they embody human interests, intentions and values; there are no value-neutral or disinterested artifacts.

Types of objectivity - (1) External objectivity - We will say that an entity, process, magnitude, construct or kind is externally objective if and only if its existence and properties are independent of its representation (2) Internal objectivity - Internal objectivity is concerned with the processes or methods by means of which science investigates the world so it might also appropriately be called methodological objectivity.

Virtually every philosopher who holds that external objectivity is the ideal toward which science is aimed also advocates an objective method that guides the process of scientific research toward its goal. So, strictly speaking, internal (or methodological) objectivists are those, such as Kuhn, Rorty, and the later Carnap, who deny that there is any coherent sense to be made of the concept of external objectivity. Lloyd (1995) attributes a double standard to philosophers who recognize, on the one hand, that the ideal of external objectivity is incoherent ... but who seem to claim, on the other hand, that there is an ideal method of science that can (and should) be value neutral.

The concept of representation-of the relationship between syntactic structures and the objective world that they represent-has been one of the most pervasive concerns of modern philosophy. The analogy of a map has been one of the most often invoked means of examining this concept. Maps, of various sorts, provide a visual instrument for "representing" the complex relationships among the syntactic, semantic and pragmatic features of representation. However, ... the map analogy fails to adequately represent the role of human agency in the process of representation. A blueprint ... is a better analogy than a map. A map is a passive representation that can be actively used to predict empirical outcomes, while a blueprint is an active representation (a projection) that plays a substantive role in forecasting empirical outcomes. The dictum "facts are theory laden" is commonly cited as a consequence of the classical pragmatists' emphasis upon the role of human expectations and human needs in guiding scientific inquiry. But the point is not simply that scientific theories constrain or condition the empirical facts that we can take to be true; more importantly, scientific theories are blueprints ... for forecasting facts they provide a framework for facts that we can make to be true. That is why it is appropriate to refer to observations as artifacts and why it is not possible to draw the sharp contrast between perception and action that is implicit in the traditional concept of representation.

Notions of scientific progress - The external realists' intuitive notion of scientific progress is clear enough: science makes progress by producing theories and data that correspond ever more closely to the independently existing, objective world. A major obstacle to this effort is that the standard ways of evaluating theories and data are all internal to the process of inquiry. Reliability is a matter of the repeatability. ... Correlation is a matter of the structural or statistical consistency or similarity of outcomes among various experimental or theoretical conditions or contexts. Questions of construct-precision,-reliability, and-correlation are internal to - objectively determinable within - the theoretical and experimental process of scientific inquiry in a way that questions of construct validity are not. Physiologists have effective methods of assessing the ... reliability of a proposed measure of toxicity. However, there is no effective method of determining whether the construct is valid-whether it really measures toxicity. The only grounds there are for the claim that the construct is valid (i.e., Really Real) lie in the internal evidence of precision, reliability, and correlation. Ultimately, these internal criteria provide the only effective measure of scientific progress.

Explicit versus implicit objectivity
The implicit "knowledge how" that one acquires through exposure to the paradigms of a practice cannot be transformed into the explicit "knowledge that" of a linguistic representation. "Norms that are explicit in the form of rules, principles, or claims (Wittgenstein's 'interpretations') depend for their intelligibility-

their determining a distinction between performances that are correct and incorrect, appropriate and inappropriate-on a more fundamental form of norms that are implicit in practice-in what is done rather than what is said . (Brandom, 1994, 62) This much of Kuhn seems right; any view that denies human reason a substantive role in science is evidently inadequate. But what seems lacking in Kuhn's account of scientific objectivity is an appreciation of the central role that algorithms or effective methods of decision making do play in all aspects of scientific research-there is an intimate connection between these ideals of coherence and the functioning of algorithms or effective methods. For example, computers can fulfill the central role that they play in scientific research because of their remarkable degree of precision and reliability. It is through such algorithms that science attains the degree of external objectivity that it possesses-the mechanical objectivity of certain aspects of formal reasoning and the mechanical objectivity of "theory independent," artifactual data.

Conclusions

Instrumental data and formal methods of testing theories-and especially instrumentation and methods that are more objective in the sense of being more automatic, mechanical, or effective-have several advantages over "subjective" protocol judgments. Perhaps most importantly, the end products of effective methods of data collection and theory testing are objective in all of the senses identified by Lloyd. ... Effective methods make it possible for scientists to accomplish immensely complex tasks-both in the collection of data and in the testing of theories-that could never be attempted .

The external, objective foundation of science is the hierarchy of artifacts that the community of competent scientists, utilizing complex (increasingly automated) experimental and computational technologies, has been able to produce (forecast). The scope of science is determined by the complex hierarchies of observational and computational artifacts that the institutions of scientific research are able to produce (forecast), together with the broad array of applied technological methods and products to which these structures have contributed. Science is not just in the business of representing (of enabling us to take true) what is Really Real: science is also in the business of constructing a system of artifacts that will enable us to more effectively achieve (to make true or to forecast) our human aspirations. At the same time, it is important to recognize that modern science embodies an objective methodology that sharply differentiates it from other social institutions. Through the development and use of more effective concepts and methods science has increased the openness and transparency of many aspects of our social and political practice. Science progresses when "higher levels of communicative discourse" are reached by transforming subjective judgments regarding the generation and reduction of data or the testing of theories into objective decision procedures that are automatic or mechanical.

IUFRO. (2004). *Task Force on the Science-Policy Interface Synthesis Workshop on the Science Policy Interface*: Meridian Institute.

This report summarizes discussions and outcomes of the Synthesis Workshop of the IUFRO Task Force on the Forest Science-Policy Interface. IUFRO established the Task Force on the Forest Science-Policy Interface (Task Force) in 1998 in recognition of the opportunity and responsibility of the forest research community to better inform forest policy deliberations. The goal of the Task Force is to explore strategies and mechanisms for improving communication between forest scientists and policy-makers so as to ensure that sound science is considered in the formulation of forest policies and on-the-ground forest management practices. The Synthesis Workshop brought together twenty-seven leading forest scientists and researchers from around the world to: (1) Synthesize findings from the case studies and identify recurring themes, (2) Identify and refine key messages for improving interactions between the forest research community and both policy-makers and society at large, and (3) Develop advice to scientists, research teams, and larger research organizations to improve and make more effective the partnerships among researchers, policy-makers and society, which is the ultimate beneficiary of forestry research. This report contains a non-attributional summary of the discussions that took place at the Workshop.

Jasanoff, S. (2004). The idiom of co-production. In S. Jasanoff (Ed.), *States of knowledge: The co-production of science and social order* (pp. 1-12). London: Routledge.

In the past 20 years, the field of science and technology studies (STS) has made considerable progress toward illuminating the relationship between scientific knowledge and political power. (i) ... The book develops the theme of co-production, showing how scientific knowledge both embeds and is embedded in social identities, institutions, representations, and discourses. ... Briefly stated, co-production is shorthand for the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it. (2) ... Scientific knowledge in particular, is not a transcendent mirror of reality. (3) It both embeds and is embedded in social practices, identities, norms, conventions, discourses, instruments and institutions ... Co-production accounts ... avoid the charges of both natural and social determinism that have featured in recent academic debates around the field of science and technology studies, including the infamous "science wars" of the 1990's. ... Science in the co-productionist framework is understood as neither a simple reflection of the truth about nature nor an epiphenomenon of social and political interests. ... As implied by the book's title ... a significant aim of several contributors is to explore how knowledge making is incorporated into practices of state making, or of governance more broadly, and in reverse, how practices of governance influence the making and use of knowledge. ... Several ... [themes] offer a means of organizing ... work in the co-productionist idiom. (5) The first has to do with the emergence and stabilization of new [concepts]: how people recognize them, name them, investigate them, and assign meaning to them; and how they mark them off from other existing entities, creating new languages in which to speak of them .... The second concerns the framing and resolution of controversy. .... the practices and processes by which one set of ideas gains supremacy over competing, possibly better established ones, or fails to do so. ... The third important line of research centers on the intelligibility and portability of products of science and technology across time, place, and institutional context. Topics under this heading range from the standardization of measures and analytic tools to the formation of new communities of practice, such as expert witnesses, who are capable of endowing claims with credibility as they are transported across different cultures of production and interpretation. ... The fourth significant tradition examines cultural practices of science and technology in contexts that endow them with legitimacy and meaning. Work in this vein has asked how the supposed universality of facts and artifacts of facts fares in disparate political and cultural settings, as well as how different domains of research and development acquire and retain particular cultural characteristics.

Submissions: Clark Miller, looking at climatic science, suggests that creating environmental knowledge about the biosphere involves not only new sciences and technologies ..., but also the fabrication of new institutions whose authority can credibly span the globe. ... Miller also shows that the attempt to supersede existing political orders produces its own tensions, exposing disagreements about the nature of "good science" as well as "good politics" on a supranational scale. ... Charis Thompson argues that the shift in the elephant's status from "endangered" to "manageable" was not due to a context specific, scientific determination of elephant biology, but went hand in hand with the emergence of a pan- African identity that could support multi-sited management practices. Originally forced to accept an absolutist scientific discourse of endangerment, African nations were enabled, through successive rounds of international negotiation, to put forward their view that elephants could be both hunted and protected in a regime of sustainable development. This "African" position, which merged ethics with science and made space for regional variation, successfully countered the monolithic bureaucratic rationality of some Western environmentalists. ... Michael Lynch directs his analysis toward the co-production of expert and nonexpert knowledges in the context of US common law trials.... Lynch's "grammatical perspective" shows how the use of ordinary words allows courts to shift between two registers: on the one hand paying homage to science transcendence by seeming to honor the categories that set science apart; on the other

hand, remaking the distinctions between science and common sense through case-centered decision making. Courts in this way perform some of the essential political work of liberal democracies by invoking and continually reproducing through their own practices the boundary between science and non-science. ... How the authority of science conflicts with or warrants other forms of authority, particularly the authority to govern at times of pronounced social change is the central theme of the three remaining chapters... Yaron Ezrahi's essay ... notes that in today's world, the representations of reality produced by science, and shared by a democratic citizenry, fight for space in the public mind with the onrush of images created and disseminated world wide by the mass media. In contrast to the esoteric knowledge and information produced at great expense by science, media representations ... are generally much more accessible to publics. They require less time, effort, knowledge and skills to interpret than does the information generated by science. ... Increasingly, Ezrahi argues, the relatively high-cost, high-entry-barrier reality of science has had to distance itself from everyday human experience; lower-cost, more accessible media realities to some extent fill the imaginative void left by the retreat of science. ... he describes ... an emergence of competing claims on the democratic political imagination, whose implications for liberty and order we are not yet in a position to asses.

Mishler, E. G. (1990). Validation in inquiry-guided research: The role of exemplars in narrative studies. *Harvard Educational Review, 60*, 415-442.

Inquiry guided research refers to the body of research that shares an emphasis on the continuous process through which observations and interpretations shape and reshape each other. It entails the dialectic interplay of theory, methods, & findings over course of study. This feature marks departure from the dominant model of hypothesis testing experimentation.

Questioning validity reformulates validation as the social construction of knowledge (417). Based on studies of scientific process that focus on actual practices of scientists rather than textbook idealizations of science, recent analyses show science as a human endeavor marked by uncertainty, controversy, and ad hoc pragmatic procedures. This is a far cry from an abstract and severe "logic" of scientific discovery. It maintains that validity assessments not assured by following procedures (418). Rather validity depends on matters of judgment and interpretation of relative importance of different threats. No abstract set of rules for assessing all levels of validity. No standard procedure can be determined either for assigning weights to diff. threats of one type of validity of for comparing different types of validity. Evaluations of validity therefore depend, irremediably on the whole range of linguistic practices, social norms and contexts, assumptions, and traditions that the rules had been designed to eliminate.

Trustworthiness is presented as a substitute for the concept of validation. Mishler argues that the essential criterion for trustworthiness is the degree to which we can rely on the concepts, methods, and inferences of a study or tradition of inquiry as basis for our own theorizing and empirical research (419). The focus is on the range of ongoing activities rather than on the static properties of instruments or scores. It adopts functional criterion (whether or not findings are relied on for further work) rather than abstract rules. This emphasizes the role scientists' working knowledge and experience plays in validation and aligns the process more closely w/ what scientists actually do.

This discovery of the contextually grounded, experience-based, socially constructed nature of scientific knowledge should be cause for celebration rather than despair (436). Does not dispense w/ methods for systematic study but locates them in world of practice rather than in the abstract spheres of Venn diagrams or Latin Squares. Skilled research is a craft learned by apprenticeship to competent researchers by hands-on experience, and by continual practice (422). This knowledge can be accumulated in part through the development of exemplars. Learning from exemplars is a process contextually grounded in practice. (436) An important task for less well-developed approaches to science is to develop a collection of relevant exemplars (437).

Pedynowski, D. (2003). Science(s) - which, when and whose? Probing the metanarratives of scientific knowledge in the social construction of nature. *Progress in Human Geography, 27*(6), 735-752.

The role of 'scientific knowledges' in the social construction of nature(s) and realities has become a focal point of deconstruction and debate in both geography and science and technology studies over the past decades. ... The critiques of the presumably dominant, and admittedly powerful, knowledge framework of 'science' are often situated within the rubrics of the sociology of scientific knowledge (SSK) or science and technology studies (STS), but also extend into debates within the geographical literature (see Cronon, 1994; Demeritt, 1994). However, these purported political aspirations have not been without challenge from individuals and knowledge communities with other political/ethical projects that are concerned with the use and construction of non-human nature(s), notably environ-mental studies and conservation biology. (Gross and Levitt 1994; Soulé 1995:137) ... I will illustrate serious errors in the characterization, definition and historical situation of ideas about 'science' in discussions of the social construction of nature/reality and implore that the assertions of science studies are more critically focused. ... I argue that many authors have constructed a metanarrative of 'science' ... [in which] science is portrayed as a homogenous activity with its 'products' implicated in various aspects of political-economic-social exploitation/oppression. This metanarrative belies the contemporary complexity of scientific endeavor and its diverse epistemic cultures. However, I do not intend to suggest the [suspension] of critical approaches to understanding the production and assessment of knowledges. All knowledges of 'nature', e.g., representations of reality, are not the same in their epistemological claims. Furthermore, while there is a multiplicity of knowledges, this does not imply likeness in their respective means of construction. In the second part of this paper, using the example of knowledges applied to decision-making in environmental management, I demonstrate that the different qualities of knowledges should be assessed with regard to their intended use in order to recognize both their positive attributes and their limitations for proposed applications. ... Sections IV and V outline a preliminary approach to clarify conflicts over competing (science-based and otherwise) knowledges. Hopefully, this simple questioning process can accommodate both the meritorious political projects of challenging empowered constructions of nature (and their potential to be used exploitatively) [the concern of the SSK and SST critiques] as well as address the equally potentially abusive situation of having limited means to assess claims to environmental realities [the concern of the conservation biologists].

In a world with multiple realities of 'nature' that are constructed by diverse groups with differing motivations and access to power, decision-making in environmental management can become a contest over whose knowledge is 'right' ... To this end, ecology as an idea and a scientific discipline has been frequently appropriated ... 'Scientific' information can take on moral authority in political and administrative forums regardless of the original context of the scientific data and theory. ... scientific interpretations, theories and hypotheses are subject to cultural, political and economic forces (Kuhn, 1970; Caldwell, 1990; Bocking, 1993). Additionally, which research questions are asked is also subject to such influences.

However, there are numerous examples in the literature of environmental policy and ecology where scientists are grappling with issues of values, choices of scientific indicators and neutrality/objectivity in their work (e.g., Clark, 1993; Budiansky, 1995; Lele and Norgaard, 1996; Lemons, 1998; Herrero et al., 2003). In an article in Conservation Biology, Lele and Norgaard (1996: 354) state that 'values, opinions, and social influences are an inextricable part of science, especially applied science'. Even Michael Soulé, ... notes that 'many nonscientists appear surprised to learn that "truth" in science is acknowledged by most mature scientists to be contingent, a matter of consensus' (1995: 168) and, in an earlier work, '[Scientific] disciplines are not logical constructs; they are social crystallizations which occur when a group of people agree that association and discourse serve their interests' (1986: 2).

Science' acts as a convenient ... foil for discursive strategies that attempt to explain and criticize particular attributes of contemporary society. The meta-narrative of powerful, unreflexive and authoritative scientific knowledges ... Such accounts appear to forget or ignore the (often blemished) legacy of other concurrently empowered knowledges, such as those of religion or ethnocentric political authority, that have also been historical forces for sanctioning 'truth', directing the development of world-views, and constructing nature(s). ... [further] it is not accurate or informative to continue this homogenization throughout 'science' - including such diverse branches of study as chemistry and psychology ... Reflecting on the internal diversity of 'science', James Bird (1989: 2) notes: "There is no agreed description of the scientific method. Just imagine the situation if there were. A totalitarian world of procedures would have to be learnt and obeyed. It is difficult to imagine such a universal framework lasting for very long."

In order to articulate what is meant by 'science' more clearly, I suggest that this body of knowledges be approached as (1) multidimensional and (2) multiscalar (to include the historicity of the knowledges, multi-spatio-temporal). ... I am suggesting a framework with which to retain a critical perspective on the production of knowledge(s) a ... there are many sciences, arranged along a continuum of methods, methodologies and claims to certainty that range from the most mathematical and logical to the most subjective and humanist approaches ... In general, scientific claims have the qualities of testability, reproducibility (or collaboration by other methods/studies, as in qualitative social research), transparency, contestability and revision in light of new findings. These qualities, while varying and evolving in their application, are arbitrated by peer (and increasingly, external) review and distinguish scientific knowledges from knowledges of a more personal or revelatory origin.

... a central aspect of the conflict is the need to equitably, openly and inclusively remedy the tensions over constructs of reality that are raised by competing knowledges. ... Some scholars of science studies have contended that all knowledges are situated, socially constructed and, therefore, none are deserving of elevated status in their claim to represent reality (e.g., Feyerabrand, 1987; Mulkay, 1992; McCarthy, 1996). ... The debates over the forms and functions of scientific knowledges will doubtlessly continue; however, regardless, policy deployment and decision-making necessitate arbitration between the knowledge/power(s) that shape the material and conceptual forms of present and future nature. Herein I will discuss an approach to analyzing competing knowledges that offers a way forward in the debate over the 'social construction of science'.

Clearly, there can be no universal answer to 'which' knowledge framework is most suitable for decision-making in any or all situations involving the future of nature(s) ... I suggest that the 'debate' between competing knowledges can be more clearly presented as a question of competing criteria for the validity of a knowledge, in the eyes of those who wish to use a knowledge for a decision-making situation. ... Judgment of appropriate knowledges in a given situation is not necessarily exclusivist, racist, colonialist or sexist. There are goals for the uses of information - those purposes should be made transparent and explicit - and the knowledge(s) chosen should reflect the purpose of their application. ... However, there are clearly situations where the decision is not so straight-forward. ... The approach to the arbitration of potentially competing landscape histories/descriptors should (ideally) be the same: the elucidation of the methods and methodologies of the knowledge production and the subsequent comparison of those to the needs and purposes of the decision. ... the valuation of these 'qualities' (i.e., criteria for validity in a given decision-making context) of scientific knowledges (or any other knowledge) is not absolute; it is ultimately dependent on the purposes and values of the users of such knowledges.

Porter, T. M. (1992). Quantification and the accounting ideal in science. *Social Studies of Science, 22*(4), 633-651.

Objectivity in science has normally been defined by scholars as almost synonymous with realism. It may be advantageous to think of it instead in terms of impersonality, an ideal that would replace arbitrariness, idiosyncrasy and judgment by explicit rules. Accounting is an exemplar of this aspect of objectivity. More important than the true representation of deep underlying financial identities is the maintenance of a system of rules that blocks self-interested distortion. Otherwise, tax codes and corporate reports would lose their credibility. From this standpoint, quantification appears as a strategy for overcoming distance and distrust. This pertains also to the natural sciences, where measurement and statistics have been crucial in transforming local experimental skills into public knowledge. We need to understand quantification as a response to a set of political problems, part of the moral economy of science. Its use in science is analogous in important ways to the explicitly political and administrative purposes served by accounting. (abstract)

I shall defend here the ... proposition that we can learn a good deal about making natural from careful study of a less exalted ... disciplined practice, accounting. (634) ... Their work is especially helpful for understanding the uses of quantification, and for constructing something like a social history of objectivity. (635) In accounting, objectivity means first of all rules. ... There is no question but that these rules must be negotiated. That negotiation brings out the problem of replication and stability in an especially acute way. ... Scientists for the most part share an interest in building stable structures of knowledge and practice.

To consider what gives strength and stability to the principles of accounting one must consider rules and entities. ... By rules I refer to purely arbitrary, unsupported conventions. Their legitimacy depends above all on their evenhandedness and inflexibility. ... Entities appeal for their authority to something like the nature of things. ... Accountants, like physicists, prefer their rules to appear to be grounded in the interests of standardization and enforceability. (636)

The Accountant's Handbook characterized 'objective' as implying 'the expression of facts without distortion from personal bias.' ... Any departure from established methods, and especially any valuation resting on nothing more than the accountants' own judgment, was likely to cause trouble. ... [But there was debate] On the one side, accounting realists challenged severely the view that anything other than truth could be really useful. (637) ... we can only claim objectivity when we know what we measure. If our objects are not defined 'it is quite impossible to speak of eliminating known biases and discovering true or estimated measures.' ... Conventional rules cannot suffice to manufacture objectivity. ... Quite different implications were drawn out next year by Wagner who wanted to see accounting as a liberal profession, like medicine. ... [a] preference for expert judgment over standardized rules. [But] this interpretive form of accounting would depend on a great reserve of trust, and it is precisely because trust is in short supply that so much emphasis is placed on accounts in the first place. (638) ... It is in large measure for broadly political reasons that a positivistic rhetoric of impersonal facts prevails in accounting. Seemingly rigorous standards are at least as valuable as claims to represent real entities in securing the faith of outsiders in accounts.

Objectivity for some accountants, was a mechanism to exclude judgment. (639) It could be 'defined to mean simply the consensus among a given group of observers or measurers.' ... Researchers applauded the quantitative form of objectivity for its amenability to empirical (meaning statistical) research, and on this account it became the consensus concept of objectivity in accounting. ... This insistence on standardizability, even where it violates the best judgment of expert practitioners, will rarely be found except in fields that are highly vulnerable to criticism from outsiders.

Still while uniform standards are highly valued, a gap between methods and reasons is an embarrassment too. (640) [but] the relationship is not normally one of conflict. ... In practice, rules are most often based on some claim to rationality. ... and despite increasingly persuasive philosophical reservations, it

has become very difficult ... to press claims to scientific rationality for what is not at least potentially reducible to rules.

It is important to note that the form of knowledge resulting from this relatively rigid quantitative protocol is decidedly public in character. ... Objectivity is a technology of distance: geographical, intellectual, and social. ... In a small-scale and unstandardized world, bargaining over measures caused no more inconvenience than bargaining over prices. ... The purposes of accounting and related forms of quantification are to be understood ... in terms of ... political values - usually some mix of justice, openness, and restraint on personal discretion. (641) ... Objectivity is synonymous with public knowledge in a deeper and more interesting sense than we have yet realized. ... Quantification provided authority, but this is authority as Barry Barnes defines it: no so much power plus legitimacy, but power minus discretion. (642)

Turning from accounting to science (643). The point ... is to notice how quantification serves as a distinctive style of communication, and promotes the formation of a certain type of scientific community. ... Many historians of science ... are now inclined to characterize science not in terms of public knowledge, but of private craft and skills. I think the appreciation of tacit knowledge ... is right and valuable... (644) [But] quantification is a form of rhetoric that is especially effective for diffusing research findings to other laboratories, languages, countries and continents. ... Why should this be so? ... Whenever a reasoning process can be made computable, we can be confident that we are dealing with something that has been universalized, with knowledge effectively detached from the individuality of its makers. This does not mean that mathematics is a neutral language, that anything can be translated into mathematics, and thereby made more precise. Quantification is a powerful agency of standardization because it imposes some order on hazy thinking, but this depends on the license it provides to leave out much of what is difficult or obscure. (645) ... Nowhere in science is the preference for mechanical and objective reasoning over the communication of complex judgments more evident than in modern uses of statistical methods to analyze experimental and observational data. As in accounting, statistical rules are not simply arbitrary, but there is a strong element of the conventional. [Example, significance levels] ... The identification of quality with levels of significance is silly. It is also impossible to justify the particular conventions of 5% and 1%. They must be understood as monuments to a scientific ethic of self-denial, as limits on what ambitious scientists can claim as positive results.

It is impossible to deny now the importance of tacit experimental skills for science. ... (646) But there is a considerable premium in science on the objective and the mechanical, often replacing personal judgment and private wisdom with standards of formal knowledge. That is, science enshrines objectivity, meaning (here) not truth to nature, but impersonality, standardization - reducing subjectivity to a minimum. This in turn has to do with the ethics of the scientific community. ... Every scientific result begins its career as a view from somewhere - say some particular laboratory - and it is really the most fundamental task of every scientist to transform as much as possible into a view from nowhere, at least nowhere in particular.

Like every community, the scientific one depends on being able to get beyond the merely personal. (647) Relatively rigid standards for reporting results, including the quantification of almost everything possible, serve science well in this regard. ... The perspective from accounting studies suggests that the social history of objectivity is a political history too, and opens up new perspectives from which the history of modern science can be related to other aspects of society, politics, and culture.

Rescher, N. (1990). Aesthetic factors in natural science. In N. Rescher (Ed.), *Aesthetic Factors in Natural Science* (pp. 1-10). Lanham, MD: University Press of America.

As Rescher (1990, p. 3) points out, although scientists may prefer simplicity (e.g., Occam's Razor, objectivity over subjectivity), nature may or may not. In fact, we view the challenges to science reviewed in the introduction as an indication that nature does not always prefer simplicity or even complexity. In many cases the nature of the problem appears to be best defined by a wickedness not amenable to approaches to science whose normative commitments are grounded in a desire for simplicity. In his discussion of simplicity as a goal of science, Rescher also addresses the second primary reason we have been slow to adopt alternative approaches to science. As he points out, although the nature of reality may not always be simplistic, there are definite advantages to pursuing a scientific approach that emphasizes simplicity. Those who pursue a simplified reality will "outdistance the latter epistemologically" because of gains due to cognitive economy. Indeed, this is what seems to have happened. Positivist paradigms have indeed outdistanced interpretivist paradigms both methodologically, and in the broader sense, epistemologically. However, given the pressures from the substantive domain which increasingly confronts a wicked reality, renewed attention is being directed at interpretivist approaches. [Summary from deleted portion of Patterson and Williams 1998]

Tognetti, S. S. (1999). Science in a double bind: Gregory Bateson and the origin of post-normal science. *Futures, 31*, 689-703.

The work of Gregory Bateson, particularly his principles for a new kind of science which, in 1958 "had as yet no satisfactory name," is revisited as a foundation for post-normal science and adaptive approaches to management of complex environmental problems. The addition of usefulness and relevance of results to decision making as quality criteria in post normal science implies inquiry into context at different levels of complexity (what Bateson refers to as deutero-learning). This in turn implies emphasis on processes that facilitate inclusion of diverse perspectives - which facilitates an understanding of relationships among different aspects of a problem; also, social learning, an adaptive approach to valuation that also inquires into the process by which values are constructed, and a reflexive approach to decision making. Though marginalized from policy discourse, Bateson's principles provided the basis for the eventual development of a new shared understanding.

Wackers, G. (1992). The chronogeography of persuasion: Normative prospects in constructivist studies. *Social Epistemology, 6*, 299-313.

Wackers points out that counter arguments against concepts like relativism are often found to be persuasive because they seem to paint an unpleasant picture of the future (one in which much is beyond our direct control and which lacks standardization). Wackers goes on to discuss and illustrate situations in which scientific perspectives have been persuasive because of what they promise to do (e.g., objective knowledge, control) independent of whether they demonstrate any capacity to do so or not. [Summary from deleted portion of Patterson and Williams 1998]

Other Literature

Cowan, G. (1998). The legal and ethical limitations of factual misrepresentation. *The Annals of the American Academy of Political and Social Science, 560*, 155-164.

All storytellers know the difficulty of telling a story truthfully. Each detail skews the description--and can, if desired, skewer the subject. ... The law of selective detail applies equally to journalists, historians, filmmakers, and academics. It would be both folly and impossible to penalize those who make their points through the use of selective detail. Indeed, great writing depends on the choice of detail. ... The principal ethical guideline, it can be argued, is the implicit contract between writers and their audiences. ... "what is at stake for the reader is the issue of whether or not a writer has violated the rules of his genre." The rules, ... are based on an unstated contract between the writer and his audience. (Malcolm 1989, 38).

... The U.S. Supreme Court has ... stated that "erroneous statement is inevitable in free debate and ... must be protected if the freedoms of expression are to have the `breathing space' that they `need...to survive"' (New York Times v. Sullivan, 376 U.S. 254, 271-72 [1964]). If the press had to worry about being sued each time it made a mistake, publishers and editors would engage in too much self-censorship and would have been forced to avoid crucial stories--from the civil rights movement, to the war in Vietnam, to Watergate, to Whitewater. ... the contract implied by libel law is that the writers will attempt to avoid making false statements of fact. The First Amendment protects virtually all speech from legal attack in order to ensure an informed citizenry. But a functioning democracy also presumes that the public will not be deliberately misinformed. As the Supreme Court has underlined, "there is no constitutional value in false statements of fact. Neither the intentional nor the careless error materially advances society's interest in `uninhibited, robust, and wide-open' debate on public issues" (Gertz v. Robert Welch, Inc., 340). ... For the news media, there is no more vital commodity than credibility. For that reason, some news organizations have codes that celebrate accuracy over speed. Good editors know that it takes years to gain credibility but only seconds to lose it.

It is possible to construct a hypothetical continuum of accuracy and balance. At one end are entities that hold themselves out as reliable sources of information on the day's events. Such outlets have the highest degree of responsibility to be balanced and accurate. Magazine pieces and works of nonfiction and history would reside at that same place on the continuum, so long as they hold themselves out as authoritative. Next on the continuum, it could be argued, are opinion pieces, ideological magazines, or books written with a clear point of view. Readers expect the facts in such publications to be, accurate, but they also know that they will be chosen selectively. Finally, one, could cut the greatest slack for docudramas or movies and plays based on fact. Here, as the Supreme Court has suggested, audiences expect some degree of dramatization... writer can, understandably, create a scene that might have happened and that is consistent with what is known about the events and characters. But writers should not invent scenes that did not happen if they distort the essence of the characters or of the story.

Empson, L. (2001). Introduction: Knowledge management in professional service firms. *Human Relations, 54*(7), 811-817.

This special issue of Human Relations seeks to make a lasting contribution to our understanding of the role of knowledge within organizations (811) . ... Post modern perspectives on organizations began to challenge fundamental assumptions about the nature and meaning of knowledge within organizations and society as a whole. These alternative perspective have taken their place in a complex and long standing debate about the role and nature of knowledge within organizations. ... Broadly, there are two alternative perspectives on knowledge in organizations: "knowledge as an asset" versus "knowing as a process."

Research which adopts the "knowledge as an asset" perspective seeks to identify valuable knowledge within organizations and to develop mechanisms for managing it effectively. ... In this context knowledge is often viewed as an objectively definable commodity, with exchanges of knowledge between individuals being governed by the functioning of an internal market. ... By contrast, researchers who adopt the "knowing as a process" perspective argue that knowledge cannot be analyzed and understood as an objective reality. Instead knowledge is viewed as a social construct ... The objective of this school of research is to understand how knowledge is created, articulated, disseminated and legitimated within organizations. Rather than consider explicit models of knowledge transmission, this perspective emphasizes the process of interaction between individuals and the role this plays in the creation, legitimation, and dissemination of knowledge on an on-going basis. ... The primary unit of analysis is the individual operating within his or her social and organizational context.

In this issue, Suddaby and Greenwood introduce yet another dimension to the discussion by providing a

field level analysis of the process by which management knowledge is produced and consumed within society. They explore two dynamics: the 'commodification' of management knowledge (i.e. the tendency of large consulting firms to reduce knowledge to a routinized and codified product) and the 'colonization' of professional fields (i.e. the tendency of the Big Five PSF's to extend commodified managerial knowledge into adjacent professional jurisdictions).

Revkin, A. C. (2004, October 19 2004). Bush vs. the Laureates: How science became a partisan issue. *New York Times*.

Andrew G. Keeler, who until June 2001 was on the president's Council of Economic Advisers and has since returned to teaching at the University of Georgia, said the Clinton administration had also played with economic calculations of the costs of curbing carbon dioxide emissions, in its case to show that limiting emissions would not be expensive ... but it made available all of the assumptions that went into its analysis, he said; by contrast, the Bush administration drew contorted conclusions but never revealed the details. "The Clinton administration got these lowest possible costs by taking every assumption that would bias them down," he said. "But they were very clear about what the assumptions were. Anybody who wanted to could wade through them." ... "He uses a Sharpie pen," said John L. Howard Jr., a former adviser to Mr. Bush on the environment in both the White House and the Texas statehouse. "He's not a pencil with an eraser kind of guy." In the campaign, Mr. Bush's team has portrayed this trait as an asset. His critics in the sciences say it is a dangerous liability. Dr. Marburger argues that when scientific information is flowing through government agencies, the executive branch has every right to sift for inconsistencies and adjust the tone to suit its policies, as long as the result remains factual. He said the recent ferment, including the attacks from the Union of Concerned Scientists, Democrats and environmental groups, all proved that the system works and that objective scientific information ultimately comes to the surface. "I think people overestimate the power of government to affect science," he said. "Science has so many self-correcting aspects that I'm not really worried about these things." He acknowledged that environmental and medical issues, in particular, would continue to have a difficult time in the policy arena, because the science was fundamentally more murky than in, say, physics or chemistry.

"I'm a physicist," Dr. Marburger said. "I know what you have to do to design an experiment where you get an unambiguous result. There is nothing like that in health and environment."